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Orogenesis on a growing Earth

Carl Strutinski

To the memory of Marin Seclaman (1938-2019) -
a great man, gifted educator and keen scientist

The end of the sixties was the time when plate tectonics was launched and only a few universities had started including the topic in their curriculum. So I had not absorbed the hypothesis when I graduated from Bucharest University in the summer of 1969. I was taught instead the then still recognized contraction hypothesis which was also causing me problems.

I can still remember a moment in my student days when, during a field trip in the Southern Carpathians, I stood for the first time in front of an outcrop, where, according to the then university assistant Marin Seclaman (1938-2019), I was supposed to see the contact between the Danubian autochthonous and the overthrusted Getic nappe. That is, a surface along which the Getic domain should have overridden the Danubian domain for umpteen kilometers. But I couldn't see it! There was no trace of the movement to describe. That was indeed frustrating, but I accepted the generally held opinion and attributed my small insight to my still very incomplete training. Years later, in one of my first papers, I was supposed to present "arguments" for the overthrust of the Supragetic domain over the Getic one (Strutinski *et al.*, 1983), a point of view which I would probably no longer agree with today. Even then I had the vague impression of denying myself just to be trendy (as new "nappes" and overthrusts were then constantly being "discovered") and to leave a good impression on Professor Mircea Sandulescu (1933-2015), the guru among Romanian geotectonicians at that time.

I first got acquainted with the hypothesis of earth expansion through Ott Christoph Hilgenberg's (1896-1976) essay "Geotektonik,

neuartig gesehen" (Hilgenberg, 1974) and Edgar Winston Spencer's book "Introduction to the Structure of the Earth" (Spencer, 1977), before I could hold in my hands in the autumn of 1979, at the place of my Polish colleague and friend Boleslaw Wajsprych, the book of the Australian Samuel Warren Carey (1911-2002) "The Expanding Earth" (Carey, 1976). For me, this was the crucial point in my further development. Not to become at once an advocate of earth expansion, but to view my findings in the field in the light of Carey's new ideas and to draw my own conclusions regarding the formation of mountain chains (orogens). It was a matter of course that I ordered Carey's book, despite the huge financial expense (the price was just under a quarter of a month's salary) in Romania, which was suffering under a dictatorship at the time. Above all, it was the realization of the existence of *transcurrent faults* running through the earth's crust over thousands of kilometers that fascinated me. Of course I had heard in lectures about the San Andreas or Great Glen faults, but these terms were much too abstract for me at that time and seemed to have only regional significance. It was only through Carey that I realized the immense importance they had in what was unfolding within the earth's lithosphere. Then there was Carey's awareness that large orogenic belts often form arches and loops, for which he coined the term *orocline*. For the first time, the subject was no longer solely megafolding, whereby a part of the crust is bent upwards and/or downwards, hence reflected in the topographical relief, but also horizontal folding, which creates the bows of a mountain belt as a whole. The mere thought of the possibility of this large-scale tectonically ductile deformation of an orogen raises doubts about plate tectonics, which, as is well known, proceeds from the assumption of interacting "rigid plates". This shortcoming has been recognized by plate tectonicists on several occasions, for example in connection with the evolution of the Malay archipelago. In this regard, Professor Robert Hall from Royal Holloway, University of London, first noticed in 1996:

> "Rigid plate tectonics may be an inadequate tool to describe the evolution of the area" (Hall, 1996),

and then repeatedly pointed out during the following sixteen years:

> "The inadequacies of the tectonic model reflect in part the difficulties of applying rigid plate tectonics, when there is clear evidence of changing shapes." (Hall, 2002); "It is … clear that the upper crust deforms in a complex way that cannot be modeled well using rigid fragments" (2012).

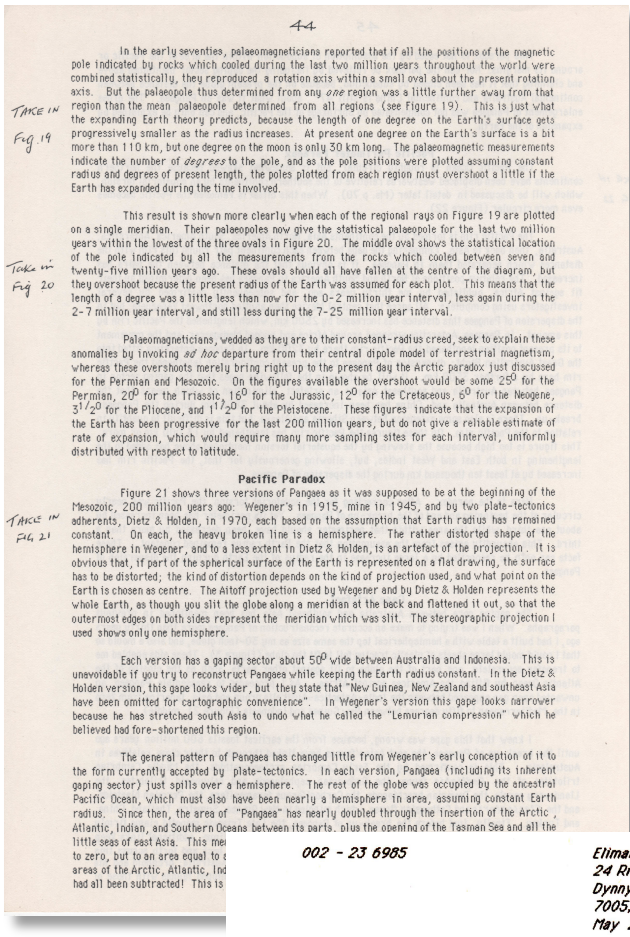
Recently, Michael Gurnis and colleagues (2018) have tried to take into account the fact of dealing with "deformable plates in the context of rigid plates" by means of a sophisticated software in order to be able to create convincing tectonic reconstructions, but these attempts are doubtful if one continues to adhere to the dogma of rigid plates.

The latter is, in my opinion, based on the erroneous assumption that the geophysically determined discontinuities at the base of the crust and/or the lithosphere represent interfaces which, from a rheological point of view, separate completely different media from one another, such as a board floating on water. However, this is not the case, because despite discontinuities the transitions are blurred, which is why a free sliding of the crust/lithosphere on the underlying mantle is physically unrealistic. In fact, one could rather be thinking of window putty, in which a thin upper layer already has hardened, but underneath the putty is still malleable and thus capable to "creep". However, I find it misleading to call the upper layer a plate, which is why I usually use this term only within quotation marks.

Another weakness of plate tectonics refers to the "motor" that is supposed to drive the movement of plates, namely the existence of convection cells acting in the mantle. As far as I know there are not too many physicists who are critical of plate tectonics, probably because most of them did not deal with the problem at all. But those who did are unanimous in their opinion that the assumption of convection cells in the mantle is physically unacceptable (Jordan, 1966; Gold, 1988; Kundt, 1998; Herndon, 2005, 2011; Tuttle, 2012). Needless to say, Carey did not admit the existence of convection cells either. Moreover, he was able to prove irrefutably that the fit of the edges of Europe/Africa and America, which had already been a topic of Abraham Ortelius (1527-1598), Francis Bacon (1561-1626) and Antonio Snider-Pellegrini (1802-1885) centuries before and which then became the cause of Alfred Wegener's (1880-1930) theory of continental drift, functions more seamlessly on a paleo-earth of smaller diameter than on that of today's scale. Thus Carey was forced to realize that the earth's body must have enlarged in geological time.

My entry into the family of Earth expansionists

Since I had now "tasted blood" through Carey's book, I was interested in whether there were other advocates of earth expansion. And so I soon came across the "globe maker from Werdau", Klaus Vogel (1926-2015), a civil engineer by profession, who caused quite a stir with his globe making, at least among earth expansionists, because his "*globe-*



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In the early seventies, palaeomagneticians reported that if all the positions of the magnetic pole indicated by rocks which cooled during the last two million years throughout the world were combined statistically, they reproduced a rotation axis within a small oval about the present rotation axis. But the palaeopole thus determined from any *one* region was a little further away from that region than the mean palaeopole determined from all regions (see Figure 19). This is just what the expanding Earth theory predicts, because the length of one degree on the Earth's surface gets progressively smaller as the radius increases. At present one degree on the Earth's surface is a bit more than 110 km, but one degree on the moon is only 30 km long. The palaeomagnetic measurements indicate the number of *degrees* to the pole, and as the pole positions were plotted assuming constant radius and degrees of present length, the poles plotted from each region must overshoot a little if the Earth has expanded during the time involved.

TAKE IN Fig 19

This result is shown more clearly when each of the regional rays on Figure 19 are plotted on a single meridian. Their palaeopoles now give the statistical palaeopole for the last two million years within the lowest of the three ovals in Figure 20. The middle oval shows the statistical location of the pole indicated by all the measurements from the rocks which cooled between seven and twenty-five million years ago. These ovals should all have fallen at the centre of the diagram, but they overshoot because the present radius of the Earth was assumed for each plot. This means that the length of a degree was a little less than now for the 0-2 million year interval, less again during the 2-7 million year interval, and still less during the 7-25 million year interval.

Take in Fig 20

Palaeomagneticians, wedded as they are to their constant-radius creed, seek to explain these anomalies by invoking *ad hoc* departure from their central dipole model of terrestrial magnetism, whereas these overshoots merely bring right up to the present day the Arctic paradox just discussed for the Permian and Mesozoic. On the figures available the overshoot would be some 25^0 for the Permian, 20^0 for the Triassic, 16^0 for the Jurassic, 12^0 for the Cretaceous, 6^0 for the Neogene, $3^1/_2{}^0$ for the Pliocene, and $1^1/_2{}^0$ for the Pleistocene. These figures indicate that the expansion of the Earth has been progressive for the last 200 million years, but do not give a reliable estimate of rate of expansion, which would require many more sampling sites for each interval, uniformly distributed with respect to latitude.

Pacific Paradox

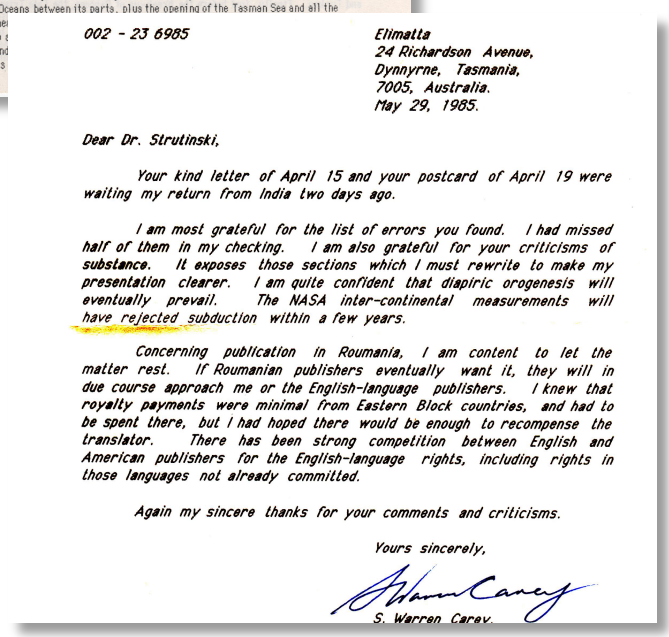
TAKE IN Fig 21

Figure 21 shows three versions of Pangaea as it was supposed to be at the beginning of the Mesozoic, 200 million years ago: Wegener's in 1915, mine in 1945, and by two plate-tectonics adherents, Dietz & Holden, in 1970, each based on the assumption that Earth radius has remained constant. On each, the heavy broken line is a hemisphere. The rather distorted shape of the hemisphere in Wegener, and to a less extent in Dietz & Holden, is an artefact of the projection. It is obvious that, if part of the spherical surface of the Earth is represented on a flat drawing, the surface has to be distorted; the kind of distortion depends on the kind of projection used, and what point on the Earth is chosen as centre. The Aitoff projection used by Wegener and by Dietz & Holden represents the whole Earth, as though you slit the globe along a meridian at the back and flattened it out, so that the outermost edges on both sides represent the meridian which was slit. The stereographic projection I used shows only one hemisphere.

Each version has a gaping sector about 50^0 wide between Australia and Indonesia. This is unavoidable if you try to reconstruct Pangaea while keeping the Earth radius constant. In the Dietz & Holden version, this gape looks wider, but they state that "New Guinea, New Zealand and southeast Asia have been omitted for cartographic convenience". In Wegener's version this gape looks narrower because he has stretched south Asia to undo what he called the "Lemurian compression" which he believed had fore-shortened this region.

The general pattern of Pangaea has changed little from Wegener's early conception of it to the form currently accepted by plate-tectonics. In each version, Pangaea (including its inherent gaping sector) just spills over a hemisphere. The rest of the globe was occupied by the ancestral Pacific Ocean, which must also have been nearly a hemisphere in area, assuming constant Earth radius. Since then, the area of "Pangaea" has nearly doubled through the insertion of the Arctic, Atlantic, Indian, and Southern Oceans between its parts, plus the opening of the Tasman Sea and all the little seas of east Asia. This me
to zero, but to an area equal to
areas of the Arctic, Atlantic, Ind
had all been subtracted! This is

Fig 1. *Page of the manuscript of the book "Theories of the Earth and Universe" (1988)*



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Elimatta
24 Richardson Avenue,
Dynnyrne, Tasmania,
7005, Australia.
May 29, 1985.

Dear Dr. Strutinski,

Your kind letter of April 15 and your postcard of April 19 were waiting my return from India two days ago.

I am most grateful for the list of errors you found. I had missed half of them in my checking. I am also grateful for your criticisms of substance. It exposes those sections which I must rewrite to make my presentation clearer. I am quite confident that diapiric orogenesis will eventually prevail. The NASA inter-continental measurements will have rejected subduction within a few years.

Concerning publication in Roumania, I am content to let the matter rest. If Roumanian publishers eventually want it, they will in due course approach me or the English-language publishers. I knew that royalty payments were minimal from Eastern Block countries, and had to be spent there, but I had hoped there would be enough to recompense the translator. There has been strong competition between English and American publishers for the English-language rights, including rights in those languages not already committed.

Again my sincere thanks for your comments and criticisms.

Yours sincerely,

S. Warren Carey.

Fig 2. *Carey's reply on learning that a Romanian publisher refused publication in Romania*

in-globe" models, in particular, showed quite clearly how earth expansion could have worked. It didn't take long and I contacted both Carey and Vogel by letter. This was mostly in the eighties, when we in the Eastern Bloc (Klaus in the GDR and me in Romania) had virtually no opportunity to travel abroad and attend congresses or symposia in order to take part in scientific exchange. Klaus admittedly had a bit more luck than I, because - even if confronted with great adversities - he finally succeeded in 1987 in obtaining the approval of the authorities to travel to Tasmania for three weeks at Carey's invitation.[1]

Meanwhile, I had gotten more and more familiar with transcurrent faults (=strike slip faults). Martin Seclaman, who was a university lecturer by that time, helped me in my undertaking by drawing my attention to the work of French petrologists, such as Adolphe Nicolas, as well as the Canadians Simon Hanmer and D.F. Strong, who shortly before had shown that frictional heat due to shearing along deep transcurrent faults can melt rocks and lead to the formation of granite magmas, which then diapirically ascended along these discontinuities (Nicolas et al, 1977; Hanmer & Vigneresse, 1981; Strong & Hanmer, 1981). In the meantime I also knew that some authors literally spoke of strike-slip orogens (Gapais & Le Corre, 1980; Badham, 1982). My first approach to see the Romanian Carpathians in this new light was presented during a symposium in Gheorghieni in 1986. The essay appeared one year later under the title: *Strike-slip faults - what are they really standing for? General features with exemplifications from the Romanian Carpathians* (Strutinski, 1987).

Two years earlier Carey had already sent me the draft of his new book[2], which he was working on, with the request to share my impressions and possibly make suggestions for improvements (Fig. 1). I considered this as a special honor and followed his request accordingly. He even asked if I would be willing to translate his book into Romanian, or if there was any interest at all in it in Romania. This led me to write to a professor of geology in Bucharest who had just published a book with a Romanian publisher to inquire about the prospects of publishing the book with that publisher as well. The professor then contacted the head of the publishing house. The answer was sobering: it would not be appropriate to upset the Romanian geologists by publishing this book directed against the

[1] It wasn't the first time Klaus and Carey met. Carey had visited Klaus already in 1979 at his home in Werdau, GDR and had seen his globe models firsthand. He must have invited him on that occasion to the symposium "The Expanding Earth" held in Sydney in February 1981. However, Klaus was denied permission to travel, so that only his article was presented there.

[2] It was the book "Theories of the Earth and Universe" which was to be published by Stanford University Press in 1988.

dominating plate tectonics theory!!! I immediately informed Carey of that message, who had to cope with this refusal (Fig. 2). By the way, the publishing director was Ion Iliescu, the one who a few years later would become the first president of Romania after the end of the communist era 1989.

My lively correspondence with Klaus Vogel, who gave me the opportunity to share with him my ideas on the possible connection between orogenesis and earth expansion, may have prompted him to encourage me to record my findings in the form of an essay and send it to Athens, where Stylianos Savvas Augustithis (1931-2001) was in the process of requesting critical contributions on plate tectonics from everywhere by means of a *call for papers*, which he intended to publish in an anthology. So I wrote a longer essay and sent it to Augustithis. In it I listed the most important weaknesses of plate tectonics (questionability of the process of the "swallowing" of oceanic lithosphere along deep-sea trenches and of all the associated, presumed, concomitant phenomena known as subduction; bias in considering ophiolites within orogens to be "remnants of swallowed old oceans"; untenability of the hypothesis on convection cells within the mantle and thus their inappropriateness as "a motor" for the displacement of plates) and established a hypothesis of mountain building that was still largely immature at that time but in line with the hypothesis of an expanding globe (Strutinski, 1990). In addition it

***Fig. 3**. My participation (front left) in the Conference "Frontiers of Fundamental Physics", Olympia (1993), where I first met personally Sam Carey (front center) and Klaus Vogel (back right).*

should be said that I would not have been able to send my contribution from the "dungeon house" Romania without having obtained at first the attestation of a reputable Romanian geologist, which I had to show when posting it. So I turned to Professor Marcian Bleahu, who had made a name for himself especially through his work in the field of speleology, but who had also just published his two-volume treatise "Global Tectonics", a Romanian counterpart to the many standard works on plate tectonics already published in the West. So I went, so to speak, into the lion's den. But I knew Professor Bleahu as an upright, unbiased free spirit and took the risk. He gave me his OK, although he could not resist telling me that by publishing my otherwise well-researched work, I was about to obstruct my scientific career. He was basically correct.

Eventually Augustithis received so many contributions that two volumes were needed to publish them all. They appeared in 1990[1].

The freedom achieved in December 1989 brought about a liberalization of scientific activity as well, but as a renegade or heretic, as I had finally come out in my Augustithis essay, I did not feel much of it. I was now able to participate in some international symposia or workshops organized by critics of plate tectonics, but I was never supported by the Romanian Geological Institute, where I had been working since 1985, and where plate tectonics was unchallenged. So I had to pay one hundred percent of the costs of my participation myself.[2] Only once was it possible for me to be sponsored by an airline company, which shared the costs of flying to Japan. It should not go unmentioned that Klaus Vogel contributed greatly to persuading me to attend the meetings in Olympia and Wrocław-Sosnówka in spite of adverse circumstances. By the way, it was in ancient Olympia that I met him personally for the first time, and Sam Carey as well (Fig. 3). Likewise, I first became acquainted there with Giancarlo Scalera, Jan Koziar[3] and Stefan Cwojdziński[4], other protagonists of the small community of earth expansionists.

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[1] "Critical Aspects of the Plate Tectonics Theory" (two volumes), Theophrastus Publictions. S.A., Athens, 1990.

[2] The conferences I attended: "Frontiers of Fundamental Physics", International Conference in Olympia, Greece, 1993; "Problems of the Expanding Earth", International Conference in Wroclaw, Sosnowka, Poland, 1994; "New Concepts in Global Tectonics", International Symposium in Tsukuba, Japan, 1998; and since 2002, the year when I moved to Germany: "Earth Expansion - A Theory put to the Test", International Colloquium in Theuern, Oberpfalz, Germany, 2003; "The Earth Expansion Evidence", International Workshop in Erice, Italy, 2011.

[3] See also the chapter by Jan Koziar.

[4] See also the chapter by Stefan Cwojdziñski.

My PhD Thesis

Formally, I had applied for a doctorate position at Bucharest University as early as 1974, but because I did not fulfill one important requirement, namely to be a member of the Communist Party, which dominated all areas of public life, another applicant was preferred at that time. So I made no further attempt to do my doctorate until the events of 1989 removed the hurdle that I could not overcome. As a result, I was able to take a doctoral position in 1990 and decided on Professor Seclaman as my supervisor. He was the one who encouraged a heretic and outsider like me by giving me his complete confidence, so that I could fully unfold my rebellious ideas. This was a huge incentive for me, which is why I will keep Professor Seclaman in appreciative memory throughout my life.[1] I only know of two other geoscientists who passed their doctorate with "expansionist topics", i.e. contrary to the "plate tectonic wave", namely James Maxlow[2] (2001) and Stefan Cwojdziñski (2003). The final statement of my doctoral thesis (Strutinski, 1997) was:

> "In conclusion, it can be stated that my findings [...] make the assumption of an expanding earth appear plausible, which however takes place under the conditions of an extremely high mobility of the lithosphere. This is to be understood in the sense that the lithosphere "moves", that is, deforms, as a result of a complex of causes among which expansion is perhaps the least decisive one. Much more important to me seems to be zonal motion triggered by the earth's rotation, which causes the differentiated mobility of the mantle [...]. Such a cause has hardly been considered so far, so that it remains reserved for future research to explore its significance for the kinematics of the lithosphere. Although the expansion is fundamentally negated by the followers of plate tectonics, I would like to point out again that it could not be refuted until today. On the contrary, preliminary geodetic measurements by NASA indicate a 2.8 ± 0.8 cm / year increase in the earth's radius (Carey, 1988). If these measurements were to be confirmed in the future, they could pave the way for complete, unquestionable recognition of the expansion of our planet."

In order to come to this conclusion, lengthy research had been necessary, spanning about the decade 1988-1997. About 350 essays and books are contained in the reference list of the first, general, part of my doctoral thesis. The authors and their works, which I perceived

[1] Unfortunately, Professor Seclaman lost his life in March 2019 in an ill-fated incident.

[2] See also the chaper by James Maxlow.

as fundamental for my scientific career and for the resulting conclusions, were, related to the different topics:

> Samuel Warren Carey (1976, 1988) concerning earth expansion;
>
> Samuel Warren Carey (1983), Johann Dorschner (1986); William N. Gilliland (1964) concerning equatorial orogens;
>
> Harold G. Reading (1980); Christopher H. Scholz and others (1979); John S. Tchalenko (1970); Arthur G. Sylvester (1988) on lateral (transcurrent) displacements;
>
> Robert Gangolf Schwinner (1924); Wallace S. Pitcher *et al.* (1985); Alain Vauchez & Adolphe Nicolas (1991) on shearing in the lithosphere;
>
> Justin Sarsfield DeLury (1941); Paul H. Reitan (1968a, 1968b, 1988); Christopher H. Scholz (1980); D.F. Strong & Simon Hanmer (1981) on frictional heat due to shearing;
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> Reginald Aldworth Daly (1917); Wallace Gary Ernst (1971, 1973, 1993); Pentti Eskola (1939); John Ferry (1992); Adolphe Nicolas & Jean-Paul Poirier (1976); Maurice Roques (1941) on rock metamorphism;
>
> Michael Ellis & Anthony John Watkinson (1987); Marin Seclaman (1982 among others) concerning orogen-parallel lineation;
>
> Philip England *et al.* (1985), Philip England & James Jackson (1989) concerning deformation of the continental crust.

One can see that the publication years of these works, which were fundamental to me, extend over three quarters of a century (1917-1992). Although I did not yet know Thomas Gold (1920-2004) at the time of writing, I can say in retrospect that I fully share his view regarding the analysis of scientific data:

> "New ideas in science are not always right just because they are new. Nor are the old ideas always wrong just because they are old. A critical attitude is clearly required of every scientist. But what is required is to be equally critical to the old ideas as to the new." (Gold, 1989)

This inviolable statement seems to be unknown to plate tectonicists by and large. Only in this way can it be explained that they do not want to know anything about classical works, with the exception of Wegener's continental drift theory or a few works by authors such as Otto Ampferer (1875-1947), Robert Gangolf Schwinner or Arthur Holmes (1890-1965), who played directly into the hands of plate tectonics through their assumptions (mountain building by

submerging crust or convection currents in the mantle). In the rather pretentious parlance of plate tectonicists this sounds like:

> "Alfred Wegener's continental drift theory, which can be regarded as a direct precursor of plate tectonics, has contributed [...] surprisingly little to the understanding of mountain building processes. Only plate tectonics was able to unite all dynamic phenomena of our earth into a unified theory and to provide explanations...". (Frisch & Meschede, 2005).

What Ampferer, Schwinner and Holmes set into the world were hypotheses, not facts. The facts, however, which many authors of the first half of the 20th century brought forward, were later either completely overlooked or dismissed as unimportant or irrelevant.

Prerequisites for my mountain-building model

I would like to give an example of overlooked facts. Daly (1917), like some authors before him, was astonished to find that in some areas, for example in the Shuswap region of the Rocky Mountains, the foliation[1] of the metamorphic rocks is parallel to the original bedding planes of the sedimentary protoliths[2]. This does not meet the expectations of either the old contraction theory or plate tectonics, for the simple reason that both assume compression. And compression must first trigger folding and only afterwards induce schistosity, whereby the parallelism between original bedding and schistosity is usually lost. Daly could only explain the parallelism by assuming a "static" metamorphism, by which rock complexes are metamorphosed simply as a result of burial to great depths, without mountain building processes[3] having to be involved. However, this view was quickly discarded by the argument that burial only increases the confining lithostatic pressure, which is unable to cause schistosity.

As far as I am aware, the first attempt to explain the parallelism between schistosity and bedding planes other than by "burial metamorphism" was made by Justin Sarsfield DeLury (1931), a Canadian geologist. He established the so-called *auto-traction hypothesis* by assuming "sheet-flows" deep in the earth that drive and carry the

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[1] Foliation is repetitive layering. Each layer can be as thin as a sheet of paper, or over a meter in thickness.

[2] A protolith is the original, unmetamorphosed rock from which a given metamorphic rock is formed.

[3] Whereby for followers of the contraction theory (example: Daly) or today for plate tectonicists, mountain building processes are axiomatically associated with compression or constriction/folding.

crust above according to the coupling principle. During progression of metamorphism more or less horizontal foliation surfaces respectively mineral lineations, are formed which indicate the direction of flow. In the following years DeLury further developed his hypothesis (DeLury, 1941). At about the same time, in his dissertation Maurice Roques (1911-1984) formulated a similar hypothesis regarding the metamorphic rocks of the French Massif Central (Roques, 1941). Both authors, as well as some others, including the Frenchman André Demay (1890-1964), are today forgotten and almost no one mentions their contributions in the field of structural geology.[1] As I shall point out, this is completely unjustified. The hypothesis of plate tectonics has long ignored the reality of horizontal schistosity and still does so today, or it interprets it in its own - not convincing - sense. Neither could it, because it is caught in its dogma, of which Carey said so aptly:

> "The deeply rooted doctrine that orogenesis implies crustal shortening is the most illusory and pernicious sophistry in the history of geology" (Carey, 1983)

The fact that plate tectonics was challenged to take up the topic is eventually due to two researchers, of whom the scientific world took little notice, since they never distinguished themselves as plate tectonicists. Instead, they preferred to keep their feet on the ground. We are talking about the Americans Lauren A. Wright (1919-2013) and Bennie W. Troxel (1921-2017). What they have achieved is unique. For they have managed to prove that the structures within the Basin and Range Province in the western United States were not created by upthrusting, meaning compression, but by downthrow, meaning stretching. Where plate tectonicists had so far assumedly seen more or less flat overthrust surfaces , Wright & Troxel (1969; 1973) "exposed" these as downthrust faults, along which formerly higher areas slid down under the force of gravity. As a consequence one had to realize now that in areas where compression was supposed to prevail, tension obviously reigned instead. And it had to be assumed, via the detour of a physically not very well-founded thermal relaxation effect, that the underground is rising incredibly fast in some areas and thus enables the deepest lower crust to reach the surface and covering rock complexes to slide downhill. The bulges of lower crust have been called metamorphic domes or *metamorphic core complexes*. It is no coincidence that some of the best-studied domes appear in the regions in which the said "unusual" foliation was detected, i.e. in the

[1] They should at least be important for the advocates of the so-called surge tectonics hypothesis (see Strutinski, 2015), but even for them these authors seem to be unknowns.

Canadian Shuswap region and in the Massif Central of France (Vanderhaeghe *et al.*, 1999). In these areas the foliation formed during the uplift of the rock piles by gliding along the already existing surfaces, which in the upper layers corresponded to the horizontal bedding planes of the former sediments.

Beyond the contradiction of the "deformable plates in the context of rigid plates", we have now also uncovered "the long-standing paradox of simultaneous extension in the hinterland and contraction in the foreland" (Vanderhaeghe & Teyssier, 1997). And there are many more of them, but we should leave it at that.

Actually, I wanted to show that the hypothesis of plate tectonics solving mountain building "simply and elegantly" is anything but as it pretends. And if any facts don't fit into its concept, it wipes them away quickly and without much ado. In this respect, I also find it significant that the English-language Wikipedia devotes a lot of space to the terms continental drift and subduction, but only handles the terms orocline and metamorphic core complex in a few sentences.

There are, of course, wide domains in which the foliation of the metamorphic rocks is not parallel to the original bedding. In most cases, the latter is no longer visible, as it has been overprinted by folding, intensive shearing and by the formation of segregated mineral bands. But even here it is not contraction that produced the foliation. As vividly described by Karl-Heinz Jacob[1] (Jacob, 2010) "ribbon-fold textures" may simply be created by self-organizing processes taking place under the action of weak electric fields. So even if rock textures appear to have been created by contraction, the arguments for this are questionable. In the case of the Alps Steiner (2014) has recently shown that it is hardly lateral compression which can explain thrusting. Schwinner (1878-1953) pointed out almost a hundred years ago that compression cannot do much in deforming rocks. On the contrary,

[1] *See also the chapter by Karl-Heinz Jacob.*

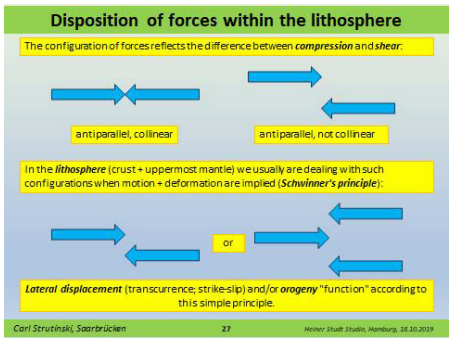

***Fig 4**. Difference between compression and shear and applicability of shear (Schwinner's principle) in the earth's crust illustrated by spatial vectors (from Strutinski, 2019)*

shear plays the central role in tectonic processes. If he still grants "normal tension", i.e. stretching, a role as a structuring factor in the uppermost part of the crust, where it "can cause tearing", he nonetheless continues:

> "Purely normal compression is the stress that achieves the least; it leads to elastic strain, not to permanent changes of shape, for the volume is strongly defended by the elastic-solid body." (Schwinner, 1924).

The simplest way to conceive shearing is to visualize paired antiparallel, non-collinear forces (Fig. 4). What is important is that, in contrast to pure stretching and/or compression, shearing mainly produces motion, i.e. it shapes decisively the kinematic setting, which is why Schwinner had no doubt that it is the normal case in geological mountain building processes. This allowed me the opportunity of coining the term *Schwinner principle*. Though the hypothesis of plate tectonics takes into consideration shearing as the primary cause in the case of large lateral displacements (by strike-slip faulting), it is the postulated compression induced by the collision of "plates" that is mostly assumed to play the main role in mountain building, shear being regarded as of only secondary importance.

Following Schwinner, I have reversed this view by assuming shearing within the lithosphere to be the utmost factor, which locally gives rise to compression or extension structures. A similar view was anticipated by Harold G. Reading (1980), who contrasted his hypothesis with that of the Wilson cycle, supported by plate tectonics. As with other ideas that ran counter to plate tectonics, Reading's hypothesis did not "catch on". For a short time it was known as the *Reading cycle* (Ingersoll, 1988; Strutinski, 1997), but it has since been completely forgotten.

The same fate was also to befall Paul H. Reitan's (1928-2011) research into the development of frictional heat along shear zones, because his finding that deformation of rocks produces heat that contributes significantly to the heat budget of orogenic belts (Reitan, 1988) contradicted the principles of plate tectonics.

My mountain building hypothesis is largely based on the above-mentioned findings, which are not recognized or rather misjudged by plate tectonics, not because they show a lesser degree of reliability compared to those which plate tectonics is operating with, but simply because they do not fit into its picture or partly contradict it. On the other hand, I have to admit that my argumentation only makes sense under the assumption of a growing earth. Consequently, I am in the situation of rejecting plate tectonics because of its weaknesses

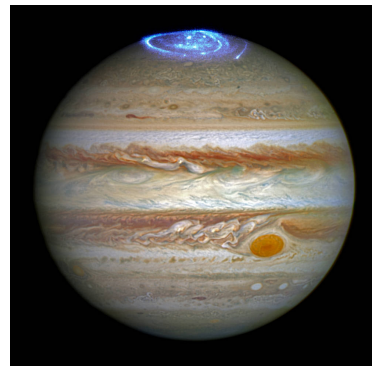

Fig 5. *The banding of the planet Jupiter (additionally with aurora borealis), which also includes vortices. Copyright NASA. (https://www.nasa.gov/feature/goddard/2016/hubble-captures-vivid-auroras-in-jupiter-s-atmosphere)*

concerning mountain building, but I do so by putting forward a hypothesis that can only be valid if the earth's body actually grows, which means that in the absence of an irrefutable proof, I have to assume earth expansion as an axiom.

The model

So what does my hypothesis imply? I start from the premise that early in the evolution of our planet, at a time when the earth expansion rate was extremely low, a continental-type lithosphere or crust existed, that covered the whole earth and that was underlain by an uppermost mantle capable of flow. By analogy with the *zonal motion* observed on the Sun and the giant planets, this upper mantle flow, tangential to the earth's surface and oriented from west to east, took place with

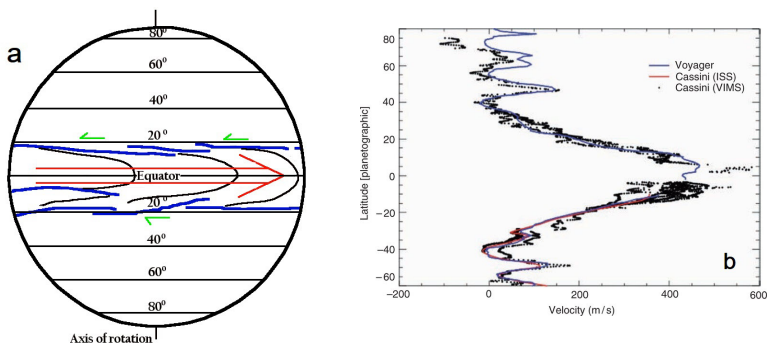

Fig 6. *The equatorial mantle current on Earth (a) may be compared most aptly with the situation on planet Saturn with regard to the ratio of angular velocities in equatorial and correspondingly tropical-subtropical latitudes (b). The graph in (b) from Choi et al., 2009*

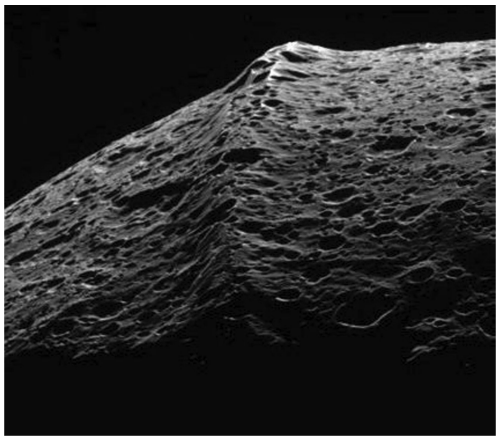

***Fig 7**. The equatorial bulge of Saturn's moon Iapetus, which almost surrounds it (from Czechowski & Leliwa-Kopystynski, 2013). Copyright NASA.*

velocities decreasing from the equator to the poles. Such motion with varying angular velocities from equator to the poles is a consequence of rotating stellar and planetary bodies. A visible result of zonal motion on the giant planets Jupiter and Saturn is their well-known banded appearance (Fig. 5).

On earth, on the other hand, I assume that there has been a sublithospheric equatorial current (Fig. 6a) whose "creep" was much faster than that under adjacent tropical and subtropical latitudes. The ratio of velocities may have been similar to that characteristic of the planet Saturn (Fig. 6b). This sublithospheric equatorial current led, by its simple operation, to mountain building within the crustal tract situated above it. It was, so to speak, the motor of mountain building, not only because of its transport capacity but primarily because of the heat generated by friction due to shearing in and above it. I have developed this hypothesis continuously over the years, starting with my lecture in Olympia 1993 (Strutinski, 1994; Strutinski, 1997; Strutinski & Puste, 2001; Strutinski et al., 2003).

Recently my friend Frank Winkelmann, a hobby astronomer, pointed out to me that Saturn's moon Iapetus was surrounded by an equatorial ridge, which might likewise have been caused by the activity of an equatorial mantle current (Fig. 7). Several hypotheses were put forward to explain this ridge. For example, it was assumed that the ridge could have formed due to gravitational differentiation of the moon's interior plus subsequent convection (Czechowski & Leliwa-Kopystynski, 2013). Thereby the main role is played by an upstream flow under the equator, acting along its whole length. According to my hypothesis, however, zonal motion, meaning horizontal and not vertical movements, would be decisive.

Relatively late I discovered that my assumptions had already been anticipated by Wenceslas S. Jardetzky (1896-1962). It was him who first spoke of zonal motion within the viscous uppermost mantle (Jardetzky, 1929). However, he assumed an earth of constant dimensions and did not consider the heat produced by friction, ultimately trying to reconcile his model with Wegener's continental drift theory (Jardetzky, 1954).

Based on Reitan's calculations, I consider that frictional heat is the cause which, in addition to the geothermal energy of the planet, triggers the metamorphic and magmatic processes within the orogens and drives them up. John Verhoogen (1912-1993) likewise had reasoned forty years ago that orogenesis should be understood as a thermal rather than a mechanical disturbance (Verhoogen, 1980).

For reasons that I have submitted elsewhere (Strutinski & Puste, 2001; Strutinski *et al.*, 2003), the equatorial sublithospheric current was dubbed *asthenocurrent* and this term shall be used henceforth. There probably was no obstacle to the flow of this asthenocurrent beneath the continental-type crust that enveloped the entire earth, since under the given circumstances the thickness of the crust would have fluctuated only slightly, without having great irregularities at its base to impede the flow. On the other hand one should bear in mind that pole migrations and associated variation of the equatorial plane could reasonably well be followed up by the asthenocurrent, but not by the overlying orogenic crustal tract. While the current would continue to "creep" at equatorial latitude, the crust on top of it would be sheared off and expelled from this position in order to clear the field for other crustal segments. Thus, in the course of time an orogenic belt would "wind up" over the mantle, giving rise to a *clew orogen* which wrapped around the earth several times and which, when traced backwards, would become increasingly older (Fig. 8).

If we now enter the respective axes of the most important orogens of the Phanerozoic (Caledonides, Variscides, Uralides, Alpides) on

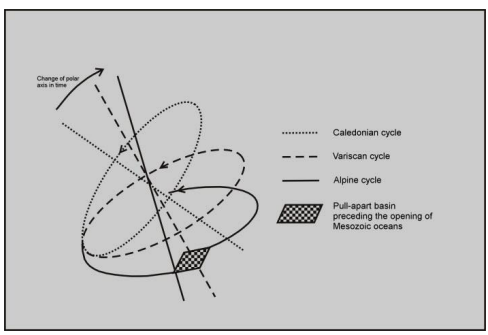

***Fig 8**. The clew orogen, which in this case shows the transition from the Caledonian to the Variscan and then to the Alpine cycle. The reason for the birth of the clew orogen is the pole migration and the related change of the equatorial plane (Strutinski, 1994).*

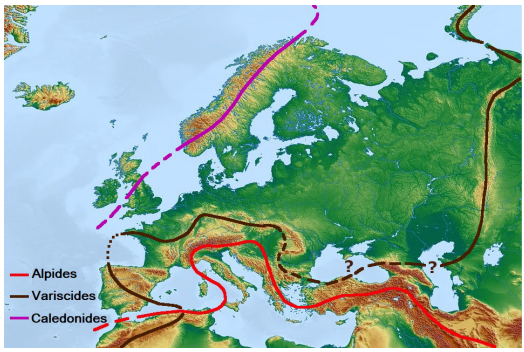

Fig 9. *The fanned out axes of the Caledonian, Variscan, Uralian and Alpine orogens within Europe, together with the oroclinal bends that mostly developed after their formation (from Strutinski, 2019. Europe's Physical Map after https://pixabay.com/de/illustrations/europa-landkarte-physische-karte-2170640/)*

Europe's physical map (Fig. 9), we can perceive their course marked by several oroclinal bends. Sometimes the traces are crossing each other or locally overlap (Fig. 9). This could never happen on the giant planets, as they do not have a solid shell on their surface. But even here, the flow is not always laminar, but often turbulent, resulting in the formation of huge vortices (Fig. 5). How much more is it to be expected that also on earth the highly viscous flow will be characterized by irregularities. Moreover, it should be mentioned that for the sake of simplicity we consider the "banks" of the orogens to be stationary, but they also experience - albeit comparatively slowly - an eastward directed movement imposed by the mantle beneath. Due to inertia, however, they lag behind their underlying mantle, which is why some authors speak of a general "westward drift" of the continents (Doglioni *et al.*, 2005; Scoppola *et al.*, 2006; Doglioni *et al.*, 2015). Such influences as well as others, caused by the creeping of the mantle, eventually produce the bending of the original straight course of orogens, old and new, leading to the formation of Carey's oroclines. Regarding the sinuous axes of the orogens crossing the European continent (Fig. 9), it is not comprehensible how plate tectonics could credibly explain such deflections. By contrast, my hypothesis explains them pretty well and might even provide indirect proof of the earth's expansion, as will be shown subsequently.

In Fig. 10 we see the initial stage of the *Variscan orogen* during the Silurian in a plate tectonic reconstruction. At this time there existed already the *Caledonian orogen*, which allegedly had caused the welding of the two shields Laurentia (LAU) and Baltica (BAL). The "welded plates" resulted in the continent *Laurussia*, also known as *Euramerica*.

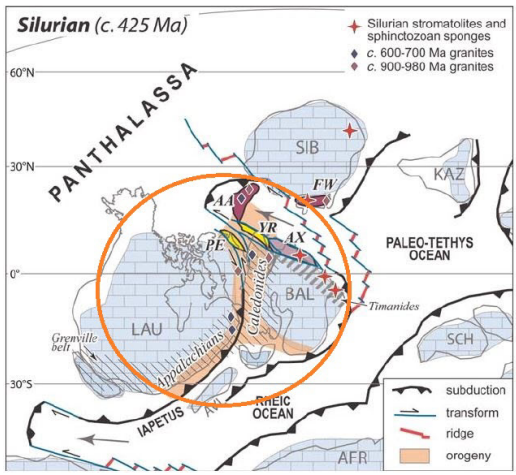

Fig 10. *Initial stage of the Variscan orogen around 425 Ma (Silurian) on an earth of today's size (according to Colpron & Nelson, 2009) If we refrain from the existence of "oceans" and connect areas, which were deformed during the Variscan cycle, we receive a circle (here rather an ellipse - orange colored), which corresponded in my view to the contemporary equator, having a radius of 3000-3500 km.*

According to plate tectonics, this "plate" was bounded in the south and east by the Rheic and Ural Oceans, respectively. The Atlantic did not exist yet. South of the above-mentioned oceans were the continents of South America and Africa, at that time still parts of the large Gondwana continent. During the Silurian the subduction of the Rheic Ocean was already in progress and not much later the same plate tectonic fate was to befall the Ural Ocean. After the "swallowing" of these oceans collision of the now adjoining "continental plates" lead to the elevation of the Allegheny Mountains in North America, the Mauritanides in West Africa, the Variscides in Europe and the Uralides on the conventional border between Europe and Asia. So much for the plate tectonic model. As can be seen from the figure, there was no parallelism between the equator and the lengths of the assumed oceans, although other reconstructions show that this parallelism existed at least in parts.

In accordance with the hypothesis expounded so far, the Variscan orogen must have formed in an equatorial position above the eastward "creeping" asthenocurrent. Subducting oceans were not necessary. I therefore assume that they did not exist. If we now connect orogenic structures of Variscan age, including those parts that have been reworked in the newer Alpine orogenesis, on a paleogeographic map, as shown in Fig. 10, we get about an ellipse or a circle. And what could

this circle represent if not the equator of a much smaller earth? The length of this circle, i.e. of the hypothetical equator, points to an earth radius of 3000-3500 kilometers. I have deliberately left the range somewhat broadly, since the determination was based only on the drawing in Fig. 10. However, the figures are within the order of magnitude assumed for the Upper Paleozoic (Maxlow, 2015). In an anti-clockwise direction, the following Variscan to early Cimmerian[1] deformed areas along my identified equator are lined up: the Ural Mountains; Pai Khoi-Novaya Zemlya; the northern part of the Taimyr Peninsula; the islands of the New Siberia Archipelago; the Wrangel Island; the northeast of Chukotka; various sections within Alaska and the Canadian Cordillera (among others the Brooks Range and Seward Peninsula in Northern Alaska, the Farewell and Alexander "terranes[2]" in Central Alaska and British Columbia); smaller "terranes" in California (Eastern Klamath and Northern Sierra); the New England Orogen in Eastern Australia; parts of northern Colombia and northern Venezuela (Sierra de Perija and Cordillera de Merida); the Ouachita Mountains and their extension to southwest Texas and north-east Mexico as well as the Allegheny Mountains in the USA; the Mauritanides in Africa; the Variscides in Europe and "Variscan parts" within the Alpides including the Caucasus. Possibly also a part of South China belongs to the "Variscan Ring". Among other supporting arguments, to which reference will be made below, it should be briefly mentioned here that the integration of the New England Orogen as well as of Southern China into the earth-spanning Variscan orogenic belt is in perfect accordance with the assumption of a growing earth, but not with plate tectonics, according to which Eastern Australia and Southern China were separated from western North America by an alleged Pacific-wide ocean (Panthalassa).

The approximate center of the described circle (Fig. 10) would have to be somewhere in southwest Greenland or in Baffin Bay and would mark the location of the North Pole at that time. This cannot be proved easily, since only old Precambrian rocks are to be found in this area, so that traces of a possible glaciation like those found on the southern continent of Gondwana, even if they existed, are most probably no longer to be found. The nearest area from which Devonian glacial deposits have recently been described is in

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[1] Cimmerian orogenesis = crustal deformation occurring chronologically between the Variscan and Alpine orogenesis. I deliberately use the term deformation instead of the usual "folding" because I believe that folding is merely a "by-product" of orogeny and should not be overrated.

[2] Terranes are so-called "exotic blocks", which seem to have been transported from outside into the area where they are found today.

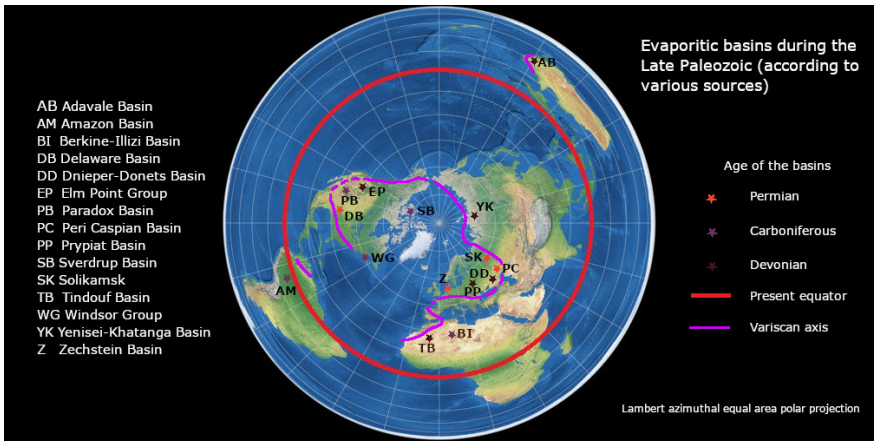


***Fig 11**. The "Variscan" equator in the Upper Paleozoic (purple), flanked by areas where evaporites were deposited (stars). Polar projection of the present Earth (equator in red). Modified after https://map-projections.net/single-view/azimutal-equal-area-gpolar*

Pennsylvania, almost 3000 kilometers to the south (Brezinski *et al.*, 2008).

It is now for the first time that I announce publicly my hypothesis about the Variscan orogen and its course around the Earth along a much shorter Upper Paleozoic equator. I think it has what it takes to initiate investigations that may help the expansion hypothesis break through. The data which helped to establish the trace of the "Variscan equator" have been brought to light in the last three decades and cannot all be enumerated here. Important were essays by Soja & Antoshkina (1997), Kiessling *et al.* (1999), Aretz & Webb (2003), Copper & Scotese (2003), Antoshkina & Königshof (2008), Gong *et al.* (2012), Laya-Pereira (2012) and Davydov (2016). These essays mainly give descriptions and show geographical distributions of Upper Paleozoic limestone reefs[1], also emphasizing paleontological similarities between regions that are today far apart, such as the Ural Mountains and parts of the North American Cordillera or northwestern South America and the Allegheny Mountains in US. On the other hand, I benefited from essays pointing to similarities between areas that today belong to Baltica/Urals, Northern Siberia and the Western American Cordillera, especially in terms of paleogeographical and paleotectonic evolution. These traits cannot be explained without significant lateral displacements along the western

[1] *According to Copper (2002), limestone reefs form at the equator and up to 30 degrees northern and southern latitudes under conditions of tropical temperatures and relatively shallow water depths that allow sufficient light irradiation.*

edge of the North American craton (Eisbacher, 1983; Wallin *et al.*, 2000; Dumoulin *et al.*, 2002; Colpron & Nelson, 2009; Miller *et al.*, 2011; Ketner, 2012). In fact, along the western margin of Laurentia a left-lateral displacement of up to 1700 km is suspected to have taken place during the Upper Paleozoic. This further supports my assumption of a Variscan east-west flowing asthenocurrent. On the whole, all these various findings give a distinctly different picture compared to that resulting from the paleogeographic reconstructions favored by plate tectonics (Scotese, 2014). These assume, as is known, a constant radius earth and often rely on unsafe paleomagnetic data. In addition, they spread the erroneous view that the tropical areas determined by means of reef limestones had extended at least during the upper Paleozoic alternatively on the northern and southern hemispheres far beyond 45 degrees, while reaching only up to about 15 degrees on the opposite sides (Kiessling *et al.*, 1999; Copper & Scotese, 2003). There is no plausible explanation for such asymmetries of the climate zones. However, these asymmetries disappear by themselves if paleogeographic reconstructions are effected on a significantly smaller globe. The assumption that the above-mentioned Variscan areas were indeed zones located on the equator or in the tropical-equatorial belt during the Upper Paleozoic is confirmed by the fact that they were flanked by areas in which evaporitic sedimentation took place during the same period (Fig. 11). It is known that the formation of evaporites requires a warm, arid climate which is found in subtropical latitudes, i.e. in the so-called horse latitudes (25-35 degrees), on both sides of the equator. These are the regions in

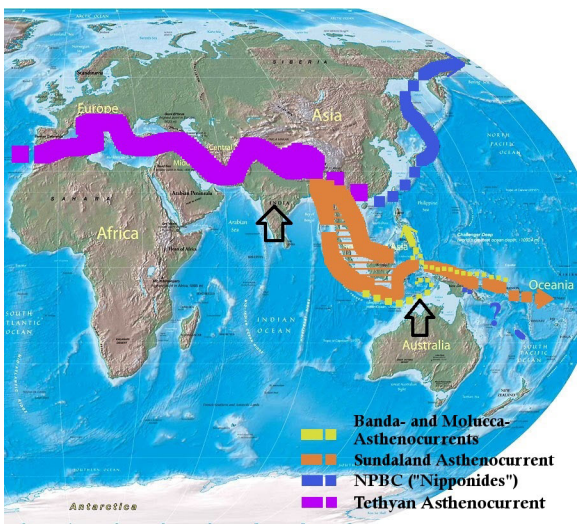


***Fig 12**. Based on the original equatorial (=Tethys-) mantle stream and the high growth rate of the earth from the middle Jurassic onwards, the North and South Pacific rim system, the Sundaland, Moluccan and Banda mantle streams have formed successively and shaped the tectonics of Southeast Asia. Background: Physical Map of the World, ©Nations Online Project*

which the vast majority of actual deserts occur. During the Upper Paleozoic they evidently formed two mirror-symmetrical zones along the Variscan axis.

Actually, the possible course of the Variscan orogen and the equalization of its axis with the Upper Paleozoic equator, cited above as an example, is a thought experiment that I made in the course of writing this essay. It may be regarded as a further step in the working out of the concept of *clew orogen*"wound up" in equatorial position. I have dealt with the Alpine tract of this orogen more closely in the past years, delivering short contributions to two physics conferences (Strutinski, 2017; 2018a). My treatise, which I finished at the end of 2018 (Strutinski, 2018b) is a provisional statement to the evolution of the Alpine orogen, especially of its Southeast Asian sector, under the premise of earth expansion. In my view, its evolution is based on the following assumption: On an expanding earth, which, since the appearance of the first oceans in the late Jurassic, has two fundamentally different types of crust, namely the "old" continental type with thicknesses up to 70 km and the "young" oceanic type, whose thickness usually does not exceed 10 km, a further "creeping" of the equatorial asthenocurrent was no longer possible. Building on this, I assumed that the rise of a huge mantle bulge at the location of the western Central Pacific, and the northward advance of India had forced the originally equatorial current to sidestep. In a first phase (Late Jurassic-Early Cretaceous) the current was constrained to split in two, one arm turning north, the other south. In a second phase (from the Eocene onwards) the remaining current was forced to turn to the right "creeping around" the Indian block and evading to the southeast where it led to the building up of Southeast Asia and Sundaland (Strutinski, 2018b). (Fig.12).

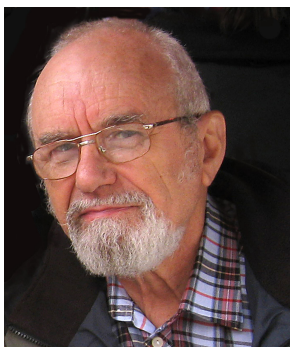
*

As far as my career is concerned, I would like to mention at the end that I have written two longer treatises in which I tried to prove that the Darwinian evolution of living beings on Earth was decisively influenced by the increase of gravitational acceleration (Strutinski, 2013 - a shorter version was published in 2012) and that the latter was also - at least partially - responsible for the large mass extinctions (Strutinski, 2016).

Epilogue

In view of the fact that there is now a large number of data - of which I have only mentioned a few - that are poorly or not at all explained by the currently most widely accepted hypothesis of plate tectonics, I find it appropriate to recommend to scientists involved in the surveying of the earth's body (physicists, geodesists) that their calculations should not be based on the preconception or axiom that our earth has not changed significantly in terms of mass and volume in the last 100-200 million years. Likewise, I find the rigid way of thinking, which I observe in various forms in supporters of both plate tectonics and earth expansion, counterproductive when it comes to approach reality. By this I mean that Heraklit's statement *panta rhei* also has its full meaning with regard to the evolution of the earth. We can assume that the velocities at which different mantle segments "are creeping" (parallel to the earth's surface) fluctuate greatly, which is why we can consider the most inert ones conventionally as "motionless". But we should never assume that the rigid crust above would not be able to *reflect* these differences. It does so by breaking, but the arrangement and specificity of the fractures provides information about the directions of movement in the creeping mantle below. The image of a "rigid" crust, as plate tectonics advocates but also most proponents of earth expansion are working with, by either reverting to collisions or by considering the crust to be passively shifted "upwards", will not be sustainable in the long run.

About the Contributor



Carl Strutinski (b. 1946) studied geological sciences at the University of Bucharest/Romania and received his Masters Degree in 1969. He worked for over 15 years in a geological enterprise prospecting and exploring, and then for almost 17 years at the Geological Institute of Romania. He was mainly involved in the study of metamorphic rocks and geotectonics. Beginning in 1987 and up to the present he published papers and longer essays including his dissertation (1997) on transcurrence processes in the lithosphere, regarding them as the primordial cause of orogeny. In doing so, he calls into question the basics of the plate tectonics theory, being a proponent of the theory of Earth growth. Since 2002 he has lived in Germany. He has been married since 1973 and has two daughters and four grandchildren.

This essay was first published as a chapter in the 2020 book, *The Hidden History of Earth Expansion*, which is widely available from good bookshops in both Hardback and Paperback editions, as well as a Google eBook.

The *Hidden History of Earth Expansion* presents the personal histories of some of the most well-known researchers into Earth expansion in 14 original essays. In addition to furnishing us with their personal histories, as they strived to explore the seemingly overwhelming evidence for confirmation of Earth expansion, the authors' highlight areas where further research is required.

The chapters expressly written for the book are:

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