Austrian Journal of Earth Sciences | Volume 107/1 | 6 - 82 | Vienna | 2014

Eduard Suess and Global Tectonics: An Illustrated 'Short Guide'

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KEYWORDS

contraction theory
history of geology
global tectonics
Eduard Suess
stratigraphy
orogeny
eustasy

Inexhaustible is the treasure of the mysteries of Nature; it represents an undescribable richness and whoever brings to light something new in this field does nothing else than opening the way to others for new researches.
Kepler

The weight of evidence for an extraordinary claim must be proportioned to ist strangeness.
Laplace

Great spirits have always encountered violent opposition from mediocre minds. The mediocre mind is incapable of understanding the man who refuses to bow blindly to conventional prejudices and chooses instead to express his opinions courageously and honestly.
Einstein

Abstract

Eduard Suess (1831-1914) is one of the most widely misunderstood and miscited authors in the history of tectonics mainly because of the nature of his writings in which very detailed local descriptions are tightly interwoven with novel theoretical interpretations. Two short publications by him, an abstract with the title *Ueber den Aufbau der mitteleuropäischen Hochgebirge* (On the structure of the middle European high mountains) published in 1873 and a letter he wrote to the editor of the English translation of *Das Antlitz der Erde*, William Johnson Sollas and which was published as the 'Preface by the Author' to the translation in 1904, may be taken as guides to probe his thinking on tectonics by finding the continuous thread running through his publications pertaining to tectonics. In fact, it is quite impossible to understand what his basic tectonic picture was without being familiar with the 1873(a) abstract, which is never cited in the literature and has not yet been examined by historians of geology. After having read it, one has to understand then why Suess stuck to the contraction theory. The answer to that is in his letter to Sollas. Basically, Suess saw that mountain-building was a consequence of motions of discrete rigid to semi-rigid lithospheric blocks moving independently with respect to one another. While a block moved to shorten its frontal part, it caused extension in its wake. Such motions of independent blocks Suess likened to the motions of ice floes in drifting pack ice. When he considered global stratigraphy, he realised that the main transgressions and regressions were global and it was them that governed the dominant character of the stratigraphic time-table. Changing the capacity of ocean basins was the only way, Suess thought, to bring about transgressions and regressions. To do this, Constant Prévost's model of global contraction (*not* Élie de Beaumont's, accepted by Dana and Le Conte) provided the best mechanism. Prévost's model worked so well for stratigraphy that Suess felt that it had to be right also for tectonics. To use Prévost's contraction for mountain-building and rift-making, Suess had to assume different depths of detachments and irregular regions of attachment of one storey to the other along such detachments. Qualitatively, Suess' tectonic model was the best ever offered before plate tectonics and plate tectonics preserved many of its basic elements and even details of some of them.

EDUARD SUESS UND GLOBALE TEKTONIK: EIN ILLUSTRIERTER 'KURZER FÜHRER': Eduard Suess (1831-1914) ist einer der am meisten missverstandenen und falsch zitierten Autoren in der Geschichte der Tektonik wegen der Eigentümlichkeit seiner Schriften, in welchen sehr detaillierte lokale Beschreibungen mit neuen theoretischen Deutungen sehr eng verwoben sind. Zwei kurze Veröffentlichungen von ihm, ein Vortragsprotokoll mit dem Titel 'Ueber den Aufbau der mitteleuropäischen Hochgebirge', publiziert 1873, und ein Brief, den er an den Herausgeber der englischen Fassung des 'Antlitz der Erde', William Johnson Sollas, schrieb und der als Vorwort zu der englischen Ausgabe im selben Jahre, 1904, erschien, können als Leitfaden zu seinem Denken über Tektonik herangezogen werden mit deren Hilfe man einen roten Faden durch seine Veröffentlichungen mit tektonischem Bezug finden kann. Es ist kaum möglich, Suess' fundamentales tektonisches Bild überhaupt zu verstehen, ohne die Zusammenfassung

von 1873(a) gelesen zu haben. Nach deren Lektüre muss man zusätzlich versuchen zu begreifen, warum Suess an der Kontraktionstheorie festhielt. Die Antwort zu dieser Frage ist in dem Brief an Sollas enthalten. Suess sah, dass Gebirgsbildung im Grunde genommen ein Ergebnis unabhängiger Bewegungen von rigiden bis semirigiden lithosphärischen Blöcken war, die sich in Bezug zueinander bewegten. Wenn ein Block sich bewegte, um an seiner Front Einengung zu verursachen, entstand an seiner Rückseite eine Zerrung. Solche Bewegungen unabhängiger Blöcke verglich Suess mit dem Driften von Eisschollen im Packeis. Als er über globale Stratigraphie nachdachte, erkannte, dass Haupttransgressionen und -regressionen global waren und dass diese den Charakter der stratigraphischen Skala dominierten. Er dachte, dass das einzige Mittel um Trans- und Regressionen zu verursachen das Variieren der Kapazität der Ozeanbecken war. Dafür bot die Constant Prévost'sche Version der Kontraktionstheorie (nicht diejenige von Élie de Beaumont, die von Dana und Le Conte angenommen worden war) den geeignetsten Mechanismus. Das Modell von Prévost funktionierte so gut im Bereich der Stratigraphie, dass Suess dachte, es sollte auch für die Tektonik gut verwendbar sein. Um das Prévost'sche Modell für Gebirgs- und Grabenbildung verwenden zu können, musste Suess verschiedene Abscherungshorizonte in verschiedenen Tiefen annehmen, entlang welcher, in begrenzten Arealen, die übereinanderliegenden Stockwerke aneinander geklebt gedacht werden mussten. Qualitativ war das globale tektonische Modell von Suess das beste, das vor dem Aufkommen der Plattentektonik vorgeschlagen worden war. Die Plattentektonik hat viele seiner Grundelemente und von manchen sogar die Details bewahrt.

1. Introduction

Eduard Suess (20th August 1831–26th April 1914) is one of the greatest geologists of all time and he is probably the greatest geologist who ever lived. If his name is no longer a household word, it is because his influence has been so enormous and so pervasive. Every geologist (and in the case of some terms such as Gondwana-Land and the Tethys much of the educated public) has heard the terms eustasy, Gondwana-Land, Tethys, foreland, listric fault, horst, graben, batholith, island arc, foredeep, Atlantic- and Pacific-type continental margins and many have heard of the Altaids, Sarmatian Stage, Angara-Land, Russian Platform (or Table), Laurentia, Caledonian Mountains, Variscan mountains, shield, back thrusting, forefolding, *Zwischengebirge* (betwixt mountains or median massifs in English translation).... We no longer reference Suess when we use these terms and most of us do not even know that he invented them. No modern textbook of geology that I know cites Suess in connexion with those terms, with the occasional exception of Gondwana-Land and the Tethys. There is a huge number of active geologists in the world today, who use the terms listed above and yet has never even heard of Suess' name (that is why a large number uses his terms and concepts incorrectly: for example, many think Gondwana-Land pleonastic and drop the word *wana* from the name of the supercontinent thereby rendering its name and what it stands for infelicitously. Others have used the term Tethys in the Caribbean, which is both untrue to Suess' meaning and both tectonically and palaeogeographically wrong even in the framework of plate tectonic reconstructions). Suess was also the one who discovered large strike-slip faults and recognised the tectonic importance of thrust faults in mountain-building on a global scale. Early in his career he became the founder of urban geology. We hardly ever think of him in these connexions any more, because they have become so commonplace. All earth scientists (palaeoclimatologists and palaeobiologists not excluded) are students of Suess as much as they are students of an Abraham Gottlob Werner or a Georges Cuvier, or a James Hutton or a Charles Lyell, or even a Darwin, but they are hardly conscious of it.

This has several reasons: number one is language. Suess wrote in German and at least since World War II, English has become the language of geology. One might have thought that at least his great classic, *Das Antlitz der Erde* (1883-1909), which is available, in addition to its original German, in English, French and Spanish, and both parts of its first volume also in Italian (but, regrettably, shorn of most of its valuable endnotes), ought to be better-known. That it is not is first because we no longer bother to reference it when we use the concepts that originated in it as I said above. Hence, both the book and its author become less and less well-remembered. The contents of that book are now truly the inhabitants of what Suess' compatriot Sir Karl R. Popper called World III, i.e. the world of independently existing intellectual products (Popper, 1979). Secondly, *Das Antlitz der Erde* is an immense book; it can be read less quickly than Hutton's great 1888 paper or than Lyell's *Principles*, which, in some of its twelve editions, was issued only in a single octavo volume (e. g., the 6th edition, 1840; 7th, 1847; 8th, 1850; 9th, 1853). In the German original Suess' classic was published in three numbered quarto volumes (but in five separate volumes numbered as Ia, Ib, II, III/1 and III/2, plus an index volume, because of the original agreement with the firm Tauchnitz that the book would consist only of three volumes) containing a total of 2788 pages. Thirdly, the publication took 26 years between 1883 and 1909, and while writing it, Suess changed his mind about a number of his own interpretations. So, a superficial reader might easily be discouraged.

The main deterrent has been, however, the way Suess wrote the book, or, rather, what he expected of his readers. The book is full of regional descriptions. So much so that many readers, even some very distinguished ones, took it as a book on regional tectonics of the earth. Bailey Willis, who hardly needs to be taken seriously, thought it just a compilation (Willis, 1930), but even Pierre Termier (1859-1930), the great discoverer of the Tauern Fenster in the Austrian Alps while on a field excursion during the ninth International Geological Congress held in Vienna, said that Suess never claims anything in that book; he just 'shows' (Termier, 1915, p. 717). I have never read a more misleading characterisation of that great book! To the contrary, the book is, what Darwin would call a 'long argument' against a particular group of theories that may be collected under the designation 'uplift hypotheses', be it those of mountain belts (propounded mainly by Leopold von Buch, 1924; Élie de Beaumont, 1829-30; Studer, 1834, 1851-53), be it those of entire continents (von Buch, 1810[1870], p. 503; Lyell, 1830-33; 1835a, b, pp. 349 and 384). Suess was vehemently against any primary vertical uplift of the lithosphere and this opposition made him blind even to the very obvious secular, ongoing, rising of Scandinavia. In *Das Antlitz der Erde* Suess indeed 'shows,' but only those things that he wanted his reader to see to underpin his claim of the invalidity of the uplift theory.

Another problem in understanding *Das Antlitz der Erde* is the local names mentioned in it (down to village or even small creek level). One needs a very comprehensive atlas at hand while reading it. Suess himself was aware of the need for an atlas to accompany the Antiltz and in the inner soft front cover* of the first part of the third volume there is a note stating that 'this second part [of the third volume] will be accompanied by a map showing the trend lines of the entire earth.' This is also alluded to in Suess' letter to Sollas reproduced below in section 3 of the present paper (see Suess, 1904, p. vii). Regrettably such a map never appeared. That is why, the Spanish translator of the *Antlitz*, Pedro de Novo y F. Chicarro, felt compelled to add four geographical maps containing 3000 geographical names to the first volume of the Spanish translation of the *Antlitz* not contained in any other version including the German original (see Suess, 1928, pp. VII-VIII). Only *Google Earth* in our days would have been adequate as an aid to read it, but the problem with *Google Earth* is that many names appearing in it are the modern ones, having been changed from the ones Suess had used for a variety of reasons, mostly political. Before *Google Earth*, only the great German atlases, such as the various editions of Andree's or Stieler's, were of much help for finding most of the places Suess mentions in his book (I still use them while reading the *Antlitz*; for a complete list of and references to these atlases, see Espenhorst, 1994, 1995, 2003). For example, all of the editions, including the five-volume mid-century edition and the last one (2011), of the famous *Times Atlas* of John Bartholomew & Son would be inadequate as an aid to read the *Antlitz*. The various editions (e.g., 1937, 1954) of the immense Atlas Mira of the Russians take an intermedite position in the detail they offer between the old German atlases and the Times atlases (but not the 1967 edition, which is of limited use if one is intersted in distances and areas, because of the purposeful distortions introduced). They too I had to consult while reading the *Antlitz*.

That does not make easy reading.

Although Suess changed his mind on many an interpretation while writing the *Antlitz*, he never deviated from his central purpose of refuting the uplift theory. That opposition is the one thread that connects all the volumes, all the chapters, almost all the pages of the book. While refuting the uplift theory, though, Suess ended up founding modern tectonics.

How did he do that? Was it his intention to found modern tectonics while writing *Das Antlitz der Erde*? Certainly not. I have no doubt that such an idea did not even cross his mind. Suess was no preacher (although his father had been trained as one). It would have never occurred to him to write such books as the *Grundfragen der Vergleichenden Tektonik* (Stille, 1924), or *The Deformation of the Earth's Crust—An Inductive Approach to the Problems of Diastrophism* (Bucher, 1933) or *Die Orogentheorie* (Kober, 1933). The books I have just cited are type examples of little gospels or law-books that their authors wrote about the final truths they believe to have discovered. Although Stille frequently said that what he published was valid '*bis zum Beweis des Gegenteils*' (until the opposite is proven) he was never able to convince himself during his long professional life (1899 to 1960) that anybody had been able to provide a falsification of any of his theses that he had established in the teens and the twenties of the twentieth century; neither was he very tolerant of those who attempted. Bucher said he proposed his laws for discussion, but he stuck to them to the end of his life (he allowed their reprinting three times: in 1941, 1957, 1963; there was also a posthumous reprint in 1968). Kober did not even bother to show that much modesty: He was sure to have discovered the *Geo-Logik* (Kober, 1946; Petrascheck, 1983, p. 25); he even wrote a little paper to tell his colleagues what the tasks of geotectonics were (Kober, 1917). These three geologists actually thought that they were the Newtons of geology and they did not seem bothered by the fact that they did not agree among themselves on many things, although there had been only one Newton and that even he had been eventually proven wrong. They characterised an episode of regression in tectonics, an episode that I elsewhere called the 'Dark Intermezzo,' spanning the interval from 1924 (when Suess' only true heir, Émile Argand, essentially withdrew from geology) to 1965 (when J. Tuzo Wilson invented plate tectonics).

In sharp contrast to these people, Suess wrote his books as invitations to argument. He even argued with himself; as I said above, he changed his mind while writing his book on many a problem. He wrote his first tectonic paper in 1868 followed by another in 1872 with a somewhat different tone from that of the 1868 paper. Already in 1873a, however, he was propounding a theory very different from what he seemed to be defending only a year earlier, closer to what he had said in 1868. Then in 1875 we see his views enlarged, only to see them partly shot down in a small paper he published in 1886, but then mainly in the *Antlitz*. This was not flippancy, but the reac-

* All volumes of the *Antlitz* were originally issued as paper-back books. They were subsequently bound by their purchasers and during the binding process the soft covers almost invariably were thrown out by the binders. This is a great pity, because some of those covers carry information not found in the text itself. Finding the *Antlitz* with the soft covers is exceedingly rare. In my entire experience I have seen only two complete sets not subsequently bound: one owned by my friend Professor Fritz Steininger in Eggenburg and the other by myself, purchased from an antiquarian book dealer in Salzburg. I own another set, in which all volumes are first editions and which originally belonged to Franz Toula. In this unique set the last volume (III/2) still preserves its soft covers, presumably because it bears Suess' autograph presentation to Toula.

tion of an honest researcher to his own ignorance and fallibility, who was interested in searching for the truth rather than in being considered an authority.

No wonder his readers were discouraged. Even some of his best students at times got lost and wrote papers against his interpretations (e.g. Bittner, 1886, 1887; Uhlig, 1907; Diener, 1916). Time, however, mostly proved them wrong and Suess right.

The question I wish to answer in this paper is whether a guide can be formulated to read Suess without being diverted into serious misinterpretations of what he said. My answer is a simple yes and, what is perhaps unexpected, is that not one, but two such implicit guides were published by Suess himself. Among the immense number of references to his writings in the literature, I have so far failed to find a single one to either of them, from which I conclude that they were either unread or their significance was not appreciated. I suppose the latter is true and therefore I shall quote them below with my own exegesis and illustrations, with the hope that they may make the experience of reading Suess' tectonic publications in the future less painful and more profitable.

But, is reading Suess still important for a professional geologist, one might be inclined to ask? Have we not already assimilated all that he wanted to tell us so that we do not have to go back to his ponderous volumes? The answer is, yes it is still important to read him and no, we have not assimilated much of what he was trying to get across. Reading Suess is necessary to understand what we do today as geologists, not to add another historical footnote to a paper or to a book we may be writing. Suess ought to be read by us, if we wish to understand the basics of our science even in a world in which plate tectonics is the basis of nearly all geological research. In 1897, the great French genius of tectonics Marcel Bertrand (1847-1907) wrote, in his preface to the French edition of the *Antlitz*, that those who understand that book would be considered the most advanced in geology. He ended his preface with the remark that sciences, like the world, are not created in one day. But, he wrote, when in the future the history of geology is written, it will be seen that the publication of Suess' book marked the end of the first day 'when there was light'. Bertrand wrote his assessment in 1897. Now, a century after Suess' death, what he wrote still stands, despite the fact that since the sixties geological theory has gone through a complete transformation.

2. Suess' first Guide: Ueber den Aufbau der Mitteleuropäischen Hochgebirge (on the structure of the Middle European High Mountains), 1873

In the gazette (*Anzeiger*) of the Academy of Sciences in Vienna, an abstract of a talk was published that Suess had given before the meeting of the Mathematical-Natural Scientific Class of the Imperial Academy of Sciences in Vienna on 24th July 1873. The abstract reads as if it were a relation by the Secretary of the Class, but it was probably written by Suess himself and given to the secretary for his report. The abstract reads (freely translated by myself; for the original German, see Appendix) as follows (My exegesis is inserted in the form of endnotes in smaller type where I deem it necessary. All inserted illustrations are mine. Suess' abstract has no illustrations as was the custom of the *Anzeiger*.):

'The full member Professor Suess presented a paper with the title "On the Structure of the Middle European high mountains." It was first shown that the opinion prevailing until now about the symmetrical structure of the high mountains and their uplifting through a central axis [Figs. 1 and 2] is no longer defensible because of many reasons, especially because a detailed study shows that with the exception of a small part of the Alps and perhaps the southernmost part of the Italian peninsula [Fig. 3], southern marginal zones do not exist in the Middle European mountain chains[1] [Fig. 6]. The newer explanations, based on the one-sidedness of mountains[2], such as those of Dana[3] and Mallet[4], correspond

Figure 1: An ideal cross-section across a mountain range according to the primary vertical uplift theory that Suess combatted and defeated. It is copied from Scrope (1925, fig. 2). I chose Scrope's cross-section, because he was such an adamant opponent of Leopold von Buch's theory of elevation craters. Yet, he clung to the uplift interpretation of large mountain ranges, just as his ally and friend Sir Charles Lyell did. Note the symmetric structure and the inconsistent vergence of the 'Limestones & Sandstones Secondary Strata.' The uplift theory did not come back in a major way (except in the writings of such authors as R. van Bemmelen and V. V. Beloussov), but the symmetric structure of mountain belts did and in Suess' own institute three years before his death, by the hand of Leopold Kober (1911, 1921), a student of Victor Uhlig.

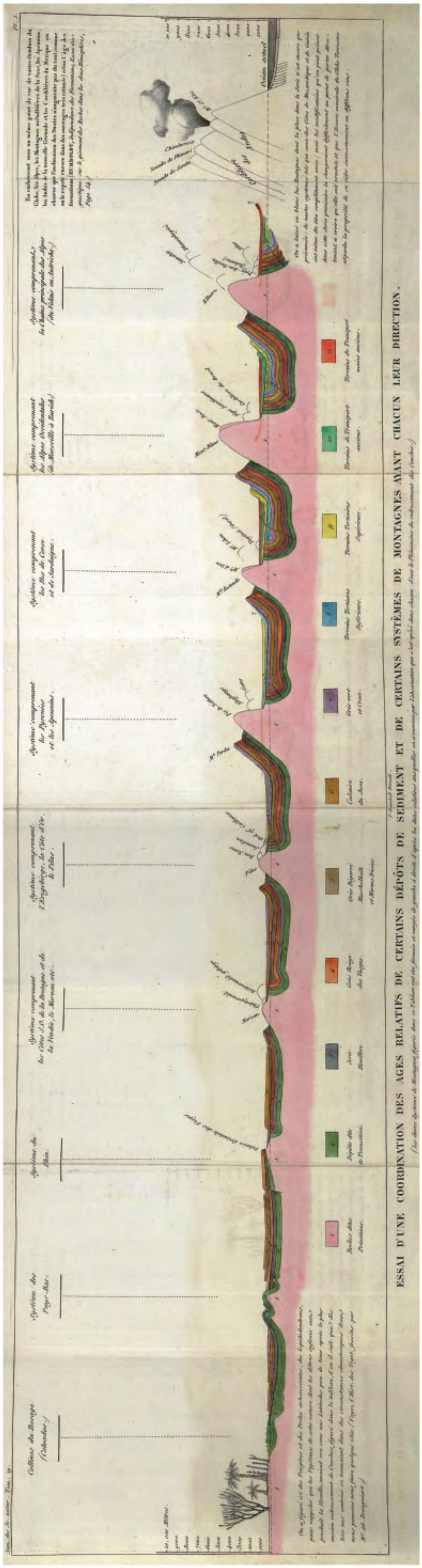

Figure 2: Schematic chronology of mountain systems and their dominant mode of formation according to Élie de Beaumont (1829-1830). Notice how the basement protrudes and turns up the superjacent sedimentary layers. Sediments after the mountain-building were laid down unconformably over the older, upturned ones. Élie de Beaumont thought that the unconformity represented the 'instant' of mountain-building which he believed was so short that it caused giant, world encircling tsunamies and major extinctions in the terrestrial biota. Notice how the youngest mountain-building is depicted to be associated with vulcanicity. Suess objected both to the chronology and the manner of mountain-building defended by Élie de Beaumont (see Şengör, 1991).

better to the circumstances, but are still not sufficient. The Alps do not fork in the inlet of Graz, as commonly said, instead the Middle European Mountains constitute, in their entirety from the Apennine to the Carpathians, a group of mountains that follow each other in the form of a fan. They exhibit regular folds towards the north or towards the northeast, but on the opposite side they show fields of extension and subsidence, volcanic constructions and earthquakes [Fig. 6].

The first of these chains that follow each other in the form of a fan is the Italian Peninsula [i.e., the Apennines]; Dalmatia with the Karst and the Bosnian mountains form the second group; the more or less east-west striking Croatian and then the Styrian chains constitute the third group; the next is already the southwest striking Bakony Forest and finally the last is the great chain of the Carpathians [Fig. 6].

The Jura and the Swabian Alb are also such chains [Fig. 6].

The trends of all of these mountains depend on the position of the older massifs and the way they are dammed against the old massifs can be recognised not only in the French Jura, in the Swiss Jura along the southern margin of the Black Forest, or in the course of the anticlines of the Austrian limestone zone south of the Bohemian Massif, but also the whole arc-shaped surrounding of the individual chains of the Western Alps, the unity of which was recognised by Desor as a consequence of damming[5].

If one regards the old massifs of Sardinia with Corsica and the Hyères, that of Central France, Central Germany and Bohemia as islands and imagines that a sea fills the space around them in which a flood wave originates in the southwest, so the trend of this wave would be entirely similar to those of the great mountain chains [Fig. 14].

The old mountains themselves seem to rend locally and to follow a similar direction, such as Riesen-[†] and Erzgebirge[‡]. Far in the east, the mountain chains seem to obey similar laws, such as the Balkan, the trachytes[6] of which had already been compared with the basalts of the Riesengebirge, with the trachytes of the Carpathians and with the volcanoes of Italy by Hochstetter[7]. Also the [Greater] Caucasus with the block at the southern point of the Crimea.

The author came to the conclusion that the entire surface of the earth is in a state of general but very slow and heteroge-

[†] Now in the Czech Republic and southern Poland forming the boundary between the Sudetes Mountains and the Bohemian Massif. They are known as the Krkonoše Mountains in the Czech Republic and the Karkonosze Mountains in Poland. Both mean giant as does the German Riese. The name derives from the magnificent erosional forms on Hercynian granites and Cretaceous sandstone (the famous Elbsandstein), which earned the place the name of 'Saxon Switzerland' (*Sächsische Schweiz*). This appellation, not a geographic, but a poetic expression, was introduced in 1786 in the *Sebnitzer Chronik* by the local Dean, later Pastor, of Neustadt, Magister Wilhelm Leberecht Götzinger (1758-1818) to make the beautiful mountains of his homeland better-known. For a geographical-touristic overview of the Saxon Switzerland, published only some decade-and-a-half after Suess worked there, see Hardenberg (1887). Also see the beautiful picture book by Paul Wolff (1924) illustrating the magnificent landscape that earned the place its name given by Götzinger.

[‡] Ore Mountains. Known as Krušné hory in the Czech Republic, meaning the same thing.

neous motion, which, in Europe, between the 40th and the 50th latitudes, is directed to the northeast or to the north-northeast[8]. The so-called old massifs move more slowly than the regions lying between them, which form chains that are dammed up. In Middle Europe, on the polar side, regular folds are built and on the equatorial side tears are produced[9].

This peculiar movement of the surface of the earth behaves, with respect to the rest of the planet, like the so-called peculiar movement of the Sun spots with respect to the rotation of the entire body of the Sun. Their direction in various parts of the earth are also various.' (Suess, 1873a, pp. 130-131).

2.1 Commentary

[1] Suess later changed his mind about the "southern marginal zone" (actually he should have said "southwestern marginal zone") of Italy, allegedly corresponding to the northern marginal zone of the Alps (Fig. 3). Fig. 4 shows his interpretation after 1875, the one we see in *Das Antlitz der Erde* (Fig. 5). As is readily recognisable, it is his later interpretation that modern geology follows to our own day.

[2] The one-sidedness of mountain ranges was not even then a new idea. Originally it applied to the topography of the mountains as perhaps first emphasised by Horace-Bénédict de Saussure (1740-1799) in his epoch-making *Voyages dans les Alpes* (de Saussure, 1796, p. 465). But that was not what Suess meant. What he meant had been first brought into a sharp focus by the truly immortal paper of the Rogers brothers (Figs. 7 A and B) William Barton (1804-1882) and Henry Darwin (1808-1866), in the Appalachians (Rogers and Rogers, 1843). In that paper the two brothers showed that the entire Appalachian structure was dominated by inclined to overturned folds with a consistent northwest vergence and by thrust faults dipping to the southeast. They interpreted this as a bodily transport of the entire mountain range towards the northwest, i.e., in the direction of the continental interior. This idea became very popular in America and James Dwight Dana (see the next note) adopted it in his *Manual of Geology* from the first edition onward (Fig. 8).

We do not know, whether Suess ever read the 1843 paper by the Rogers brothers (Rogers and Rogers, 1843). However, in the Library of the Museum of Natural History in Vienna there is a copy of the *Reports of the First, Second and Third Meetings of the Association of American Geologists and Naturalists, at Philadelphia in 1840 and 1841, and at Boston in 1842, embracing its Proceedings and Transactions*, in which the paper of the Rogers brothers was published. We also know that this book had been there before Suess was employed by the *Hofmineralienkabinett* (the ancestor of the present *Naturhistorisches Museum* in Vienna) from Paul Partsch's (1791-1856) catalogue of the library (Partsch, 1851, p. 169, no. 2674). I can hardly believe that the voracious reader Suess, who was at the time working on brachiopods, which had necessitated a review of the Appalachian literature, and who was also responsible for cataloging the library books and found that this 'mechanical' task in fact had impressed in his memory many titles that later had come in handy (Suess, 1916, p. 146), would have not read that book. In any case, he does cite in *Die Entstehung der Alpen* (Suess, 1875), a later revised and enlarged version of the same paper (Rogers, 1859c, pp. 884-916), with better illustrations, included in Henry Barton Rogers' great classic *The Geology of Pennsylvania* (Rogers, 1859a, b, c, d). In that later publication all the main arguments of the earlier paper are repeated and, in addition to the text-figures not present in the original paper, the gorgeous coloured geological map of Pennsylvania and a separate map of the anthracite basin make it very easy to follow the arguments of the paper in detail. Nobody who has read Henry Darwin Rogers' book can remain unimpressed about the importance, nay the great dominance, of asymmetric horizontal motions in mountain-building and I cannot imagine that Suess had not

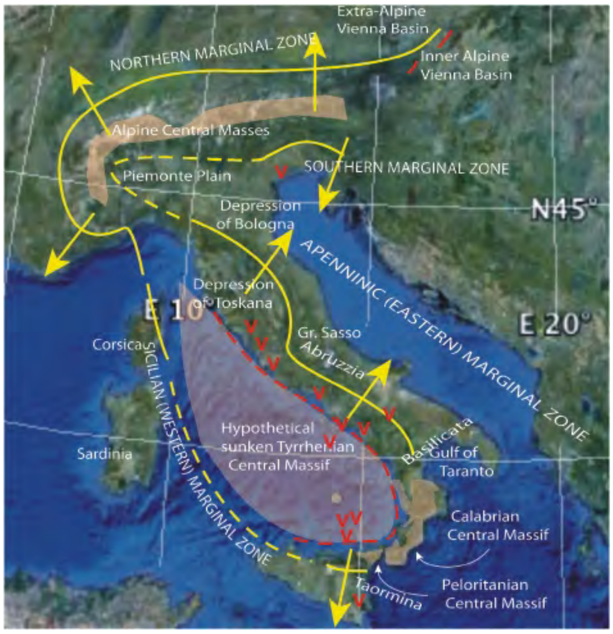

Figure 3: Map showing Suess' interpretation of the structure of the Apennines and the Alps in 1872. Note that both mountain ranges are seen to be symmetric and the Sicilian folds and thrusts southwest of the Peloritani Massif are regarded as the equivalent of the northern Alpine marginal zone. Suess assumed that a large sunken Tyrrhenian Massif was the real continuation of the Alpine central massifs southwards. By 1873, Suess had changed his mind, and regarded both mountain chains as asymmetric, but in the *Entstehung* we still read of a small fragment of a marginal zone (implied is northern marginal zone!) near Taormina in Sicily (Suess, 1875, p. 28)! In his memoirs, Suess says that it was in the Basilicata that he first conceived the idea of the asymmetry of the Apennines and the Carpathians and says that his travelling companion Professor Gerhard vom Rath (1830-1888) describes in his book relating their joint excursion there how much he was impressed between the Sila Massif and the Basilicata (Suess, 1916, p. 233; see also Şengör, 2009a). But all vom Rath remembered was how much Suess had been impressed with the similarity of the metamorphic rocks and the placement of the calcareous chain in the Basilicata to the Alpine central massifs and the Northern Calcareous Alps near Innsbruck (vom Rath, 1871, pp. 136-137; see also Şengör, 2009a). However, Suess may have later reinterpreted in his mind the significance of the inverted folds of the Basilicata and then believed that he had changed his interpretation right there and then. At least this is not what is seen from his publication of 1872. Base image is from Google Earth. V's represent Quaternary volcanoes, arrows the vergence.

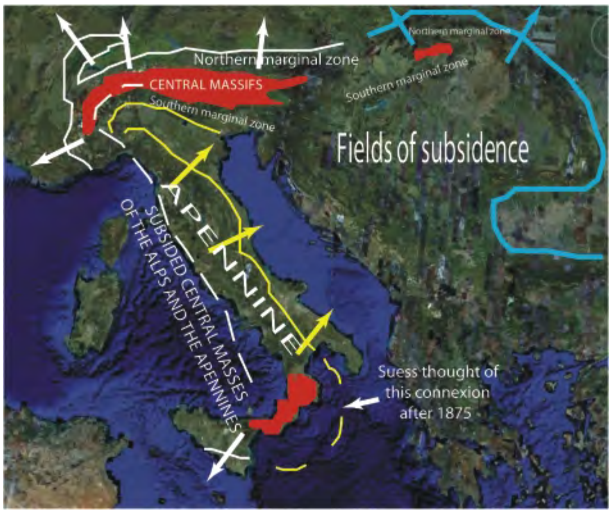

Figure 4: Suess' reinterpretation of the structure of the Apennines, the Alps and Carpathians as asymmetric ('one-sided' as he was fond of saying). This full interpretation first was published in the *Antlitz* (see the next figure). Base image is from Google Earth. Arrows show vergence.

been excited by it. For the development of Suess' ideas on the one-sidedness of mountain belts and their evolution and sources, see Şengör (2009a).

We know that the importance of the one-sidedness of mountain belts was one of the central tenets of Suess' interpretation of global tectonics and remained so to the end of his days. When his grandson, the great physical chemist, nuclear physicist and cosmochemist Hans Eduard Suess (1909-1993), was giving his acceptance speech for the Leonard Medal of the Meteoritical Society in 1977, he said 'When I was a little boy, I was told all about continental drift and plate tectonics [sic!], and how mountains were folded asymmetrically. Later however, I was told by others that this was fantasy.' (Marti and Wänke, 1985, p. 291). Here Hans Suess no doubt refers to the conversations of his geologist father Franz Eduard Suess (1867-1941) with his grandfather Eduard. They could not have been talking about plate tectonics then, but the idea that mountains are folded asymmetrically must have made a strong impression on grandson Suess' young mind (Fig. 9). What he also made sure to stress was that others later, during what I elsewhere (in Şengör, 1998) called the 'Dark Intermezzo' (1924-1965), told him that all this was fantasy. Hans Suess

Figure 5: Eduard Suess' pencil sketch of the main trend lines of the Alpine System (from Suess, 1883, fig. 26, p. 303)

must have felt happy for his father and grandfather when, after plate tectonics came about, most of their ideas, which he had heard as a child, were corroborated. The great similarity of what was talked about at home when he was a little child and what he heard as a mature scientist must be the source of the non-geologist Hans Suess' confusion about his father's and grandfather's conversations including 'plate tectonics'.

[3] James Dwight Dana (1813-1895, Fig. 10). American geologist, professor of geology in Yale University from 1850 to 1892. He was also first the co-editor with his father-in-law, the famous American geologist Benjamin Silliman (1779-1864) and then the sole editor of the prestigious *American Journal of Arts and Sciences* (later *American Journal of Science*). Dana wrote three fundamental textbooks that exercised a profound influence on geology in the whole world. The first was his *System of Mineralogy* (1837), which continued publication with different appendices, authors and editors until it reached its seventh edition in the sixties of the twentieth century. His *Manual of Mineralogy* (1848) continues to be published, again under different authors and editors to our own day. The last edition is the 23rd, called *Mineral Science* (*after James D. Dana*) and written and edited by Cornelis Klein and Barbara Dutrow in 2007. Finally, his *Manual of Geology* was first published in 1863 (Dana, 1863) and had four editions, the last being in 1895. With unnumbered revised and corrected editions published by different publishers this famous book had six different editions, all by Dana himself alone. It is very possible that Suess first learnt Dana's ideas on tectonics from the first edition of his textbook. It was already present in the library of the *Hofmineralienkabinett* in 10th January 1863 having arrived there very shortly after its publication in December 1862 (written communication by Andrea Kourgli, *Naturhistorisches Museum*, Wien, via Thomas Hofmann, *Geologische Bundesanstalt*, Wien, 23rd October 2013; for the publication date of the first edition of the *Manual*, see Prendergast, 1978, p. 531). Dana had a great influence, not only on Suess' thinking in tectonics, but also on the thinking of all the 20th century Kober-Stilleans (for the Kober-Stillean school of thinking, see Şengör, 1982a and b), who followed Dana much more closely than Suess ever did.

[4] Robert Mallet (1810-1881, Fig. 11) Irish geophysicist and engineer, one of the founders of seismology (he coined the term). Mallet acquired a great and well-deserved fame because of his studies and catalogues on earthquakes and very especially through his meticulous and epoch-making study of the Neapolitan earthquake of 16th December 1857 in which he delineated the earthquake zones of the Mediterranean and of the whole world. Suess mentions his name here mainly in connexion with his paper *Volcanic Energy: an Attempt to develop its True Origin and Cosmical Relations* published in 1873 in the *Philoposphical Transactions of the Royal Society* (London). In that paper, Mallet comes out forcibly against the notion of primary uplift as a cause for raising mountain chains and continents, because, as he rightly points out, such uplift from below would only create stretching above it. He makes a refe-

rence to Dana to argue that it had long been established that 'the great continents have not been the work of such elevatory forces at all, but have resulted from the deformation of a cooling and contracting globe covered only by a thin and yet flexible solidified crust, sinking over great areas and relatively or absolutely rising over others, has been so convincingly argued by Dana and other American geologists that it is probably now admitted.' (Mallet, 1873, p. 154). The main point of Mallet's paper was to show that the contraction-related compression and crushing of rocks would suffice to create melting in the crust and hence feed igneous action, including the volcanoes. Suess must have found all these ideas most attractive as they seemed to harmonise perfectly with his own observations and deductions.

[5] Pierre Jean Édouard Desor (1811-1882, Fig. 12) German geologist of Huguenot ancestry. He first studied law, but because of his participation in the famous Hambach Festival events (27th to 30th May 1832), during which ideas leading to the eventual 1848 revolution were aired, he was pursued by the police and had to flee to Paris, where he became a student of Élie de Beaumont. The effects of Élie de Beaumont's teaching are clearly visible in his first book on the geology of the Alps in which he talks about a grand catastrophe that elevated the Alps and led to important changes in the animal and plant 'economy' of the time of its occurrence (Desor, 1862, p. 65). He later became assistant to Louis Agassiz (1807-1873) while the latter was working on his great work on fishes. Desor also helped him with his glaciological studies and during that time met the great Scottish physicist and glaciologist James David Forbes (1809-1868). Most significant for his later rôle in understanding the structure of the Alps, was Desor's participation in the famous Pennsylvania Survey of Henry Darwin Rogers when he was in the United States between 1847 and 1852 during which he fell out with Agassiz (Gerstner, 1994, note 24 on p. 270, claims that the fault lay with Desor, on the basis of Lurie's biography of Agassiz; however, recent research and the new availability of personal archives have shown that the situation was not nearly so clear-cut as Lurie alleges and Gerstner accepts: see Kaeser, 2004, p. 110, footnote 1) and became very close friends with Rogers (Gerstner, 1994, pp. 160-161, 178-179). This shows that Desor was intimately familiar with the ideas of the Rogers brothers. In fact, Kaeser (2004, p. 153-154) points out that Desor was seduced by the thesis advanced by Rogers that all the wonderful folds they were mapping in the Pottsville coal basin together in the company of the great Swiss palaeobryologist Léo Lesquereux (1806-1889) were products of lateral shortening as shown by the inclination of the fold axes and actual fold overturning. At the time Desor began wondering whether the same idea could not be applied to the Jura Mountains in Europe. He even published two papers in 1853 and 1855 about this subject after he returned to Switzerland (see Kaeser, 2004, p. 154, footnote 1) and before he published his books on the Alps.

By mentioning Desor's name in his abstract, Suess actually makes a reference to Desor's second book on Alpine structure (Desor, 1865a, also see Desor 1865b), and particularly to the following passage and the map (Fig. 13), where both Élie de Beaumont's influence ('systems of elevation') and Desor's liberation from some of it (chains of different trends being of the same age) can be seen (passage freely translated by Şengör):

'Position and distribution of the central massifs in groups or systems of elevation'

Although it is a characteristic of the central massifs to appear solitary and independent of one another, it cannot be claimed that there is no connexion between them. It appears to us to be obvious that their position with respect to one another obeys certain laws, which one can recognise without great difficulty as long as one is not under the influence of prejudices.

One such law necessarily strikes us when we take a look at the attached map [Fig. 13; and Desor's coloured folded geo-

Figure 6: The Central European High Mountain Ranges according to the description in Suess (1873a).

logical map at the end of his book]. It is the very remarkable, continuous connexion of the sedimentary rocks carrying the same designation as opposed to the limited extent of the crystalline central massifs. The connexion of the sedimentary rocks is maintained in all the different directions of strike and dip. So, for example, it is the same zone of Jurassic rocks coming from the Bernese Alps that crosses the Rhone near St. Moritz, surrounds the Montblanc and strikes towards us at the chain of Belledonne and cutting the Tarentaise and the Maurienne streching up to Mont Viso and thus describing a curve of one fourth of a circle. The change in the direction, however, causes no essential change in the character of the rock types. It is obvious that this zone acquired its form and direction through a single elevation, which gave it the shape of a curve. It is thus not a result of the intersection of many elevations along straight lines, as one believed for a long time [This is where Desor criticises the theory of his teacher Élie de Beaumont, without naming him. For Élie de Beaumont's theory here criticised, see Élie de Beaumont (1829-1830, 1830a, 1831, 1833, 1835). Suess also rejected Élie de Beaumont's theory of mountain-building].

When we observe the position of this wide [sedimentary] zone with respect to the central massifs more closely, their significance appears more precisely. It is this: together with the anthracite and Triassic formations, the crystalline cores come between the Alpine lands on both sides and separate the Alps of Piemont from those of Dauphiné. But it also provides connexion between the individual crystalline cores rising like islands from it. In order to make the relations of the crystalline massifs to one another clearer we have tried to connect the central massifs of the Western Alps with one another according to their position and propinquity in the attached sketch [Fig. 13]; for a coloured version of this map, see Desor, 1865b, plate III. This shows three curved zones or belts of crystalline breakouts, which are fairly parallel with one another and can be seen as many, probably simultaneous, waves of rising crystalline massifs during the elevation of the Alps.' (Desor, 1865a, pp. 76-77).

Especially in the very last sentence, it is impossible not to see the direct influence of the ideas of the Rogers brothers

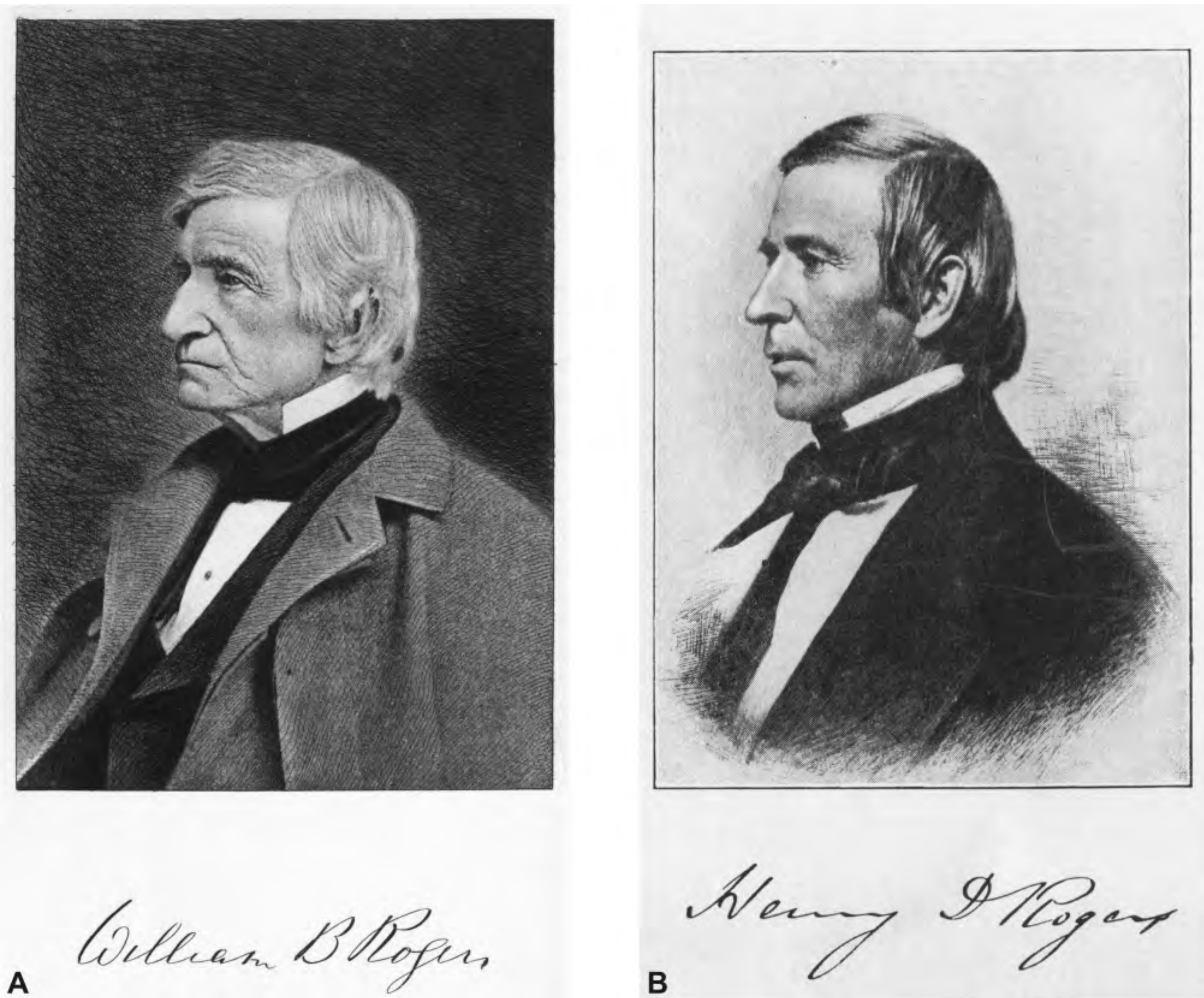

Figure 7: In my estimation, two of the four greatest geologists that America has ever brought forth (the others are Grove Karl Gilbert (1843-1918) and J. Tuzo Wilson (1908-1993)) and forerunners of Eduard Suess in elucidating the internal structure and manner of formation of mountain ranges of our globe: A. William Barton Rogers B. Henry Darwin Rogers.

on Desor. The Rogers brothers had also viewed the Appalachian folds, including those of the inner crystalline zone, as simultaneously generated parallel folds as a result of catastrophic explosions to their southeast. Desor seems to have learnt this kind of thinking from Henry Darwin Rogers and I venture to think that it was the influence of Henry Darwin Rogers that tore him away from the spatial aspects of the tectonic theories of his teacher Élie de Beaumont. The catastrophist views of Élie de Beaumont were not in necessary conflict with those of the Rogers brothers, so Desor hung on to them.

[6] By trachyte Suess here means mainly andesite but also in places the rock described as trachyte is rhyolite. The reason for this confusion is, as Harry Rosenbusch (1836-1914) (1898, p. 288, footnote 1) pointed out, that the term andesite was first introduced by Leopold von Buch (1836, p. 190, [1885], p. 308; von Buch had read his paper before the Academy of Sciences in Berlin on 26th March 1835: see von Humboldt, 1858, p. 633), in order to separate the albite-containing varieties of the trachyte family of his time from the sanidine-containing ones. Von Buch himself never used the term again (von Humboldt, 1858, p. 636) and later the term was dropped in favour of trachyte alone, because the definition, von Humboldt wrote, should have specified not albite with hornblende, but oligoclase with augite. In 1861 Justus Roth reintroduced it for the younger volcanic rocks, in which oligoclase-amphibole or oligoclase-augite were dominant (Roth, 1861, p. XLV, footnote 1) in order not to stretch the meaning of trachyte too far. However, as is common in geology, the term trachyte continued long after Roth's redefinition of andesite to be used for rocks which we today would call andesite and even rhyolite. In some cases, the reverse happened too: Tyrell (1921) pointed out, for example, that Daly's (1914) computations of the average composition of andesite were such that they would even include rocks called trachyte today! Von Humboldt's (1858) long and detailed endnote 85 on his pp. 633-636 gives an excellent summary of the fortunes of the term andesite until 1858, just three years before Roth finally resurrected it.

[7] Christian Gottlob Ferdinand Ritter von Hochstetter (1829-1884) Austrian (originally from Württemberg) geologist and explorer. He was a student of Friedrich August von Quenstedt (1809-1889) in Tübingen together with the famous palaeontologist and stratigrapher Albert Oppel (1831-1865). In the early fifties he was employed by the Austrian Imperial and Royal Geological Survey in Vienna together with Eduard Suess and Ferdinand Freiherr von Richthofen (1833-1905), the great explorer of China (see note 30 to the next section). While in the employ of the Survey, von Hochstetter studied the Cainozoic basalts of Bohemia. He was later appointed as the geologist of the Austrian world-encircling *Novara* Expedition, but left the expedition in New Zealand to explore its geology. His monumental three-volume work on the geology of New Zealand (the second and the third volumes were written by other specialists, including Suess, using the material von Hochstetter had collected) became the foundation stone of the geology of those islands. Upon his return to Austria he became professor of geology in the Vienna Polytechnic Institute (from 1815 to 1872 k. k. Polytechnisches Institut; from 1872 to 1975 Technische Hochschule and since 1975 Technische Universität Wien: see Neuwirth [1925]; Nöbauer, 1979) later becoming the director of the Natural History Museum in Vienna, a position he occupied until his early death. In the Summer 1869 he explored the geology of European Turkey, which, in those days, extended all the way from the Bosphorus to beyond Belgrade. The work Suess refers to is the fruit of that trip, which was originally undertaken 'for practical interests', mainly of mining and raw materials (von Hochstetter, 1870; see also Akyol, 1940, pp. 537-538, but especially Tollmann, 1996, pp. 374-376). The specific passage Suess refers to is the following (freely translated by me):

'This steep southern slope of the Balkan formed through a great dislocation, along which the mountain regions south of the Balkan, which once connected the Balkan plateau with

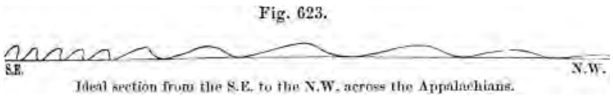

Figure 8: Cross-section across the southern Appalachians showing the basic principle of their deformation (from Dana, 1863, p. 405).

Figure 9: Eduard Suess and his grandson, the future great cosmochemist and nuclear physicist Hans Eduard Suess in the garden of grandfather Suess' summer home in Suessstrasse 8, Marz, Burgenland. It was in this environment in which the younger Suess has heard of asymmetric mountain structure, continental drift and many other concepts, which, he was later told, were all wrong. Hans Suess lived long enough to see that those ideas he had heard as a child were all right after all.

the southern Thracian highlands, the Rhodope or the Despoto Mountain, subsided apparently during the Tertiary, during the period of the immense trachyte eruptions. The subsided parts of the mountains, where not covered by the extensive surfaces of the Subbalkan basins of Slivno, Kizanlik, Karlova and Sofia, now form the M i d d l e M o u n t a i n s between the Balkan and the Rhodope, the Karadja Dagh, the Sredna Gora and the Ihtiman Middle Mountains' (von Hochstetter, 1870, p. 399, emphasis by spacing is von Hochstetter's). Suess refers to this exact passage also in *Die Entstehung der Alpen*, p. 47, note 40 (Suess, 1875). For the comparisons by von Hochstetter (1870) between the 'trachytes' of the Balkan with those of the inner Carpathian, i. e., Pannonian, basins, see pp. 381 (comparison with the Hungarian and Transylvanian volcanics) and p. 393 (with the Hungarian 'greenstone trachytes'). Under 'trachytes' von Hochstetter here means not only andesites, but also rhyolites and also some basalts. Remarkably, von Hochstetter had immediately recognised the importance of young normal faulting here, which was long forgotten until rediscovered later in the late twentieth century (see Tzankov et al., 1996 and Roy et al., 1996)! For his work in Turkey, von Hochstetter was decorated by the Ottoman Sultan Abdülaziz (1830-1876; reigned 1861-1876) with the Mecidiye Medal, third class, in 1871.

[8] The great significance of this statement, from the viewpoint of the way Suess worked, becomes obvious in the framework of another, published two years later in *Die Entstehung der Alpen*, where Suess describes how he built successive models and refuted them in turn during the course of his studies, in a way arguing with himself. This is what Karl Popper described as critical rationalism. Suess was a critical rationalist, in the full sense of the phrase, throughout his life (Şengör, 2006). Referring to the idea of the northerly movement of the European surface, he later wrote:

'In what a wonderful manner does Nature refute our assumptions! ... After we have given up a geometric system and accepted the one-sidedness of the movement, we find a dominant uniform northward striving in many mountain ranges, considered old or young, from the Cordillera to the Caucasus. We would like to formulate a law of flow of the upper part of the earth towards the pole. But this is also wrong. Farther to the east follow some dislocations along the meridians, then the moving force turns south in the mighty high mountain ranges of inner Asia. We thus obtain a picture of the face of the earth which does not at all correspond to our expectations of regular beauty, but so much more to the truth.' (Suess, 1875, pp. 145-146).

The real significance of what is said in this sentence by Suess is, however, that until that time no one had said anything so totally new on the basis of such a meticlous research. He was here talking about the wholesale motion of a continent-size part of the upper rocky rind of the earth. Such a thing had never been said before in serious geological literature. True, in 1660, Bernhardus Varenius (1622-1650/51?) had remarked, in his book *Geographia Generalis*, which is now regarded as the foundation stone of general geography, that in the past America and Europe had been a single continent and America was later torn from Europe and therefore the American Indians were also children of Adam (Varenius, 1660, p. 333); the American Richard Owen [Currey] (1816-1865) published the idea in 1857 that the entire American continent had once formed an upper layer on top of the western part of the Old World and had slid off it to open the Atlantic Ocean (Owen, 1857, especially p. 75) and Australia had similarly slid off Arabia; in 1858, the French geographer Antonio Snider[-Pellegrini] (1802-1885) believed that the Atlantic had been rifted open during the Biblical Flood (see Şengör, 2014). None of these fanciful 'theories' had found an audience among the geological community, however, and they were rightly laughed off.

What Suess was saying, however, was a different matter. He had carefully considered the available data and his statement was a hypothesis that seemed best to explain what he saw. What he said was testable and could not be laughed off.

[9] Here Suess talks about the relative motion of what he calls 'old massifs' with respect to one another. What he means is depicted in Figs. 15 and 16, after his descriptions in Die *Entstehung der Alpen*. What these figures reveal is quite amazing: Suess saw the relative motion of blocks in the Alpine foreland with respect to one another with extensional boundaries behind them and shortening in front of them. In the *Antlitz* he also introduced the third kind of boundary: strike-slip (e.g. Suess, 1883, p. 163, fig. 12; also see Suess, 1913). In *Die Entstehung der Alpen*, he likened this tectonic style to pack ice (Suess, 1875, p. 156). This is a recognition that was to be rediscovered by Franz Lotze (1903-1971) (1937) in exactly

Figure 10: James Dwight Dana as depicted by Daniel Huntington in the middle of the nineteenth century.

the same region as Suess first formulated his tectonic ideas, then rediscovered by İhsan Ketin in Turkey (Ketin, 1948) and finally rediscovered again on a large scale by Tuzo Wilson (1908-1993) (1965). It is extremely revealing to compare the figures of Lotze (1937; herein Fig. 17) with those of Wilson (1965; herein Fig. 18) and then read Suess 1873a, 1875 and 1883 again! The third rediscovery by Wilson we call plate tectonics. Plate tectonics is nothing but the combination of Suess' tectonics, Wegener's continental drift and the recognition, by Wilson, of the fact that Suess' tectonics can be applied also to the ocean floors. Wilson ended the Dark Intermezzo in the history of tectonics in the twentieth century by reviving Suess with Alfred Wegener's spectacles. He was able to do this, because we had finally learnt the tectonics of the ocean floors, a realm that had remained essentially out of Suess' reach.__

3. Suess' second Guide: Preface by the author [to the Face of the Earth, english edition of *Das Antlitz der Erde*] (1904)

'DEAR PROFESSOR SOLLAS,

I am extremely gratified by the news that the Clarendon Press[1] intends to do me the honour of issuing an English edition of my book *Das Antlitz der Erde*, and that this is to appear under an *aegis* so distinguished as yours[2]. The honour is so much the greater since the first volume had already appeared in 1885, the second in 1888, and the first half of the third in 1901. With the lapse of time so much has been accomplished in the exploration of distant regions, as well as in the more detailed study of countries already known to us, that the reader will meet here and there in the first two volumes with a description already antiquated. In a comprehensive work, however, which is devoted not to the formulation of laws, but to the comparison of observations scattered over the whole earth, and the endeavour to establish connexion and correspondance between them, deficiencies of this kind can never be entirely avoided, certainly not when the advance of knowledge is so rapid as at present. A criticism of previous views must in addition be the natural result of this method[3].

Now, however, you must permit me to say a little more concerning myself than would under other circumstances be consistent with the laws of modesty.

In the year 1849 my geological investigations began in the Archaean[4] and Silurian[5] districts of Bohemia[6]; in 1850 I proceeded to the Alps; and in 1851 I was commissioned to make a section across the Dachstein mountains, one of the lofty masses of Trias limestone[7] in the eastern Alps. The contrast to Bohemia, with its vast rounded masses of gneiss and horizontal sheets of Quader sandstone[8], was particularly striking[9]. In 1854 I became acquainted in Switzerland with Bernhard Studer and Arnold Escher von der Linth[10]. Escher with all his simplicity was a remarkable man. He was one of those possessed of the penetrating eye, which is able to distinguish with precision, amidst all the variety of a mountain landscape, the main lines of its structure. He had just come forward with the magnificent conception, unheard of in the views of that

Figure 11: Robert Mallet.

time, of a double folding of certain parts of the Alps, which has since received the name of the "double fold of Glarus"[12]. Studer opposed him. Such movements of the mountains were, he said, contrary to nature and inexplicable. Escher did not concern himself with the explanation, but with the facts[13].

A few years later I had the good fortune to make the acquaintance of Sir Charles Lyell[14], with whom, as with Escher, I maintained friendly relations till the close of his life. On the one side stood Sir Charles, the calm superior philosopher, the lucid thinker and clear writer; on the other dear old Arnold Escher, who entrusted his admirable sketches and diaries to every one indiscriminately, but to whom every line he had to publish was a torment, and who was perhaps only quite in his element up in the snow and ice, when the wind swept his grey head and his eye roamed over a sea of peaks. In characterizing this time I only mention these two important men, because in the contrast of their qualities the whole wide field of activity in our glorious science is brought into view. Lyell's *Principles*, however, of which the ninth edition appeared in 1853, while investigating at length many fundamental questions, scarcely touched that of mountain-formation: Escher would not enter at all into the discussion of theoretical questions[15].

In 1852 Élie de Beaumont's[16] 'Notice sur les systèmes des montagnes' [*sic*!] appeared in a form not very easily accessible, the *Dictionnaire universel d'histoire naturelle*[17]. The geometrical theories put forward found little appreciation outside France[18]; apart from these, however, the treatise contained many new and suggestive views on the origin of mountains[19]. Nevertheless the idea of the dependence of the more recent on the older folding found no expression in it, and when I thought of the contrast between Bohemia and the Alps one of the few beacon-lights appeared to me to be the profound words of Sir Henry de la Beche published in the 1846 in the first volume of the *Memoirs of the Geological Survey of Great Britain*, p. 221: '*that the foldings of the mountains of South Wales correspond to adaptation to a complicated lateral pressure*.'[20]

Let me now review rapidly the subsequent decennia in which geological opinions underwent step by step a profound change[21]. They were modified in England as elsewhere. I have observed with great pleasure that as early as 1873 you yourself put forward in a Syllabus of a Course of Lectures on Geology[22] the two principles that earthquakes may proceed from tectonic processes[23], and that the buttress of earlier Palaeozoic land may have had considerable influence in determining the direction of the folds in South Wales.

The theory of elevation craters fell to the ground[24]. 'What,' it was asked, 'is meant by elevation?' The classical example, the temple of Serapis[25] (Fig. 39) and the Baltic sea[26], even presupposing the complete correctness of the facts, illustrated only locally limited processes, and it was not possible to explain by them the undeniable fact that the terminology of formations, created in England, might be applied over the whole globe [27]. Some universally active causes must exist. The formation of mountains, however, evidently belongs to quite another series of phenomena, and the horizontal strand-lines which may be observed above the existing sea-level are quite independent of the anticlinals and synclinals of the slope along which they extend.

Figure 12: Édouard Desor.

If we consider one of the most regularly built and most exactly known of mountain ranges, the Jura, we shall find nothing that would correspond to a central axis of elevation in the older sense of the term. The dependence of the processes of mountain-formation on older elements which check and oppose, on lateral pressure and deflexion, become daily more evident (see Figs. 44-47), as for instance in the thrusting of the northwest of Scotland over the gneiss of the Hebrides. Thus our very first consideration of existing observations brings us back to the felicitous expression of accommodation to lateral pressure. Now we understand why the Alps extend towards the Carpathians in a concave curve, avoiding the older Bohemian mass, and we are led to a comparison of the mutual relations between the great structural units.

Even in 1885 and 1888, the dates at which the first volumes of this work appeared, the possibility was recognized of deducing from the uniform strike of the folds of a mountain-chain a mean general direction or trend-line; such trend-lines were seen to be seldom straight, but as a rule arcs or curves, often violently bent curves of accommodation; the trend-lines of central Europe were observed to possess a certain regular arrangement and to be traceable in part as far as Asia (see Şengör et al., in this volume). It was further recognized that the ocean from the mouth of the Ganges to Alaska and cape Horn is bordered by folded chains, while in the other hemisphere this is not the case, so that a Pacific and Atlantic type may be distinguished[28]; that the Mediterranean is not part of the Atlantic Ocean, but the remains of a sea which once crossed the existing continent of Asia, and has since been enlarged by subsidence[29]: that at various times, as for instance during the middle and upper Cretaceous period, an extension of the seas occurred much too general and too equable to be explained by the subsidence of continental land, and so on. All these results completely crushed the hope of arriving at a geometrical plan of the face of the earth, but the desire to discover whether some plan of another kind might not determine the distribution of mountain ranges received a fresh impulse. In 1888, however, it was not possible to further pursue this path. Important English investigations had been published on the Himálaya; Java was known; Richthofen had explored China[30]; Japan was becoming accessible[31]. The results of all these important researches, however, and the map of the curves formed by the islands of eastern Asia show a number of peripheral, chiefly arc-shaped fragments surrounding a vast and wholly unknown centre situated in Mongolia and Siberia, the exploration of which could alone furnish continuity.

The third volume had already advanced far towards completion within the limits originally proposed when an examination of these very regions was commenced on a large scale preparatory to the construction of the Siberian Railway[32]. At the same time a number of distinguished geologists combined in Paris under the direction of MM. Marcel Bertrand and Emile[§] de Margerie with a view to publishing a French edition of the *Antlitz*, and this flattering circumstance strengthened me in the resolve to extend my task before its conclusion. Meanwhile Teleki and Höhnel had discovered lake Rudolf in Africa, and with it the Ethiopian rift valley[33]; Széchenу and Loczy[34], Obrutschew[35] and others[36] had explored central Asia and enabled us to bring the trend-lines of Burma into connexion with those of the great ranges of the interior; the united efforts of French and Swiss geologists in the western Alps led to conceptions which went much further than Escher's double fold[37], received with so much incredulity at the time; Törnebohm pointed to lateral movements in Scandinavia of no less importance[38]; through the devotion and insight of our American colleagues the remarkable structure of Alaska became increasingly clear. From all sides came fresh information.

The first half of the third volume is thus mainly devoted to Siberia and the relations of the inner Asiatic trend-lines. The second half will contain the conclusion of the descriptive part, and some chapters of synthetic matter which refer to the whole planet. In this a plan of the trend-lines of the earth will be found. It will be a first attempt, burdened by all the difficulties, perhaps too all the errors, arising from this circumstance, but it will have fully accomplished its purpose if it is found fit to serve as a link to the fresh observations which unceasingly succeed one another.

I remain, esteemed colleague,

Yours very sincerely,

E. SUESS.

VIENNA: *January*, 1904.'

3.1 Commentary

[1] The Clarendon Press, as Suess calls it, is actually the academic wing of the Oxford University Press at the Great Clarendon Street in Oxford. It is the world's second oldest (after Cambridge University Press, which received its letters patent from Henry VIII in 1534; Oxford received its regal endorsement in 1586, although it had already been in the printing busi-

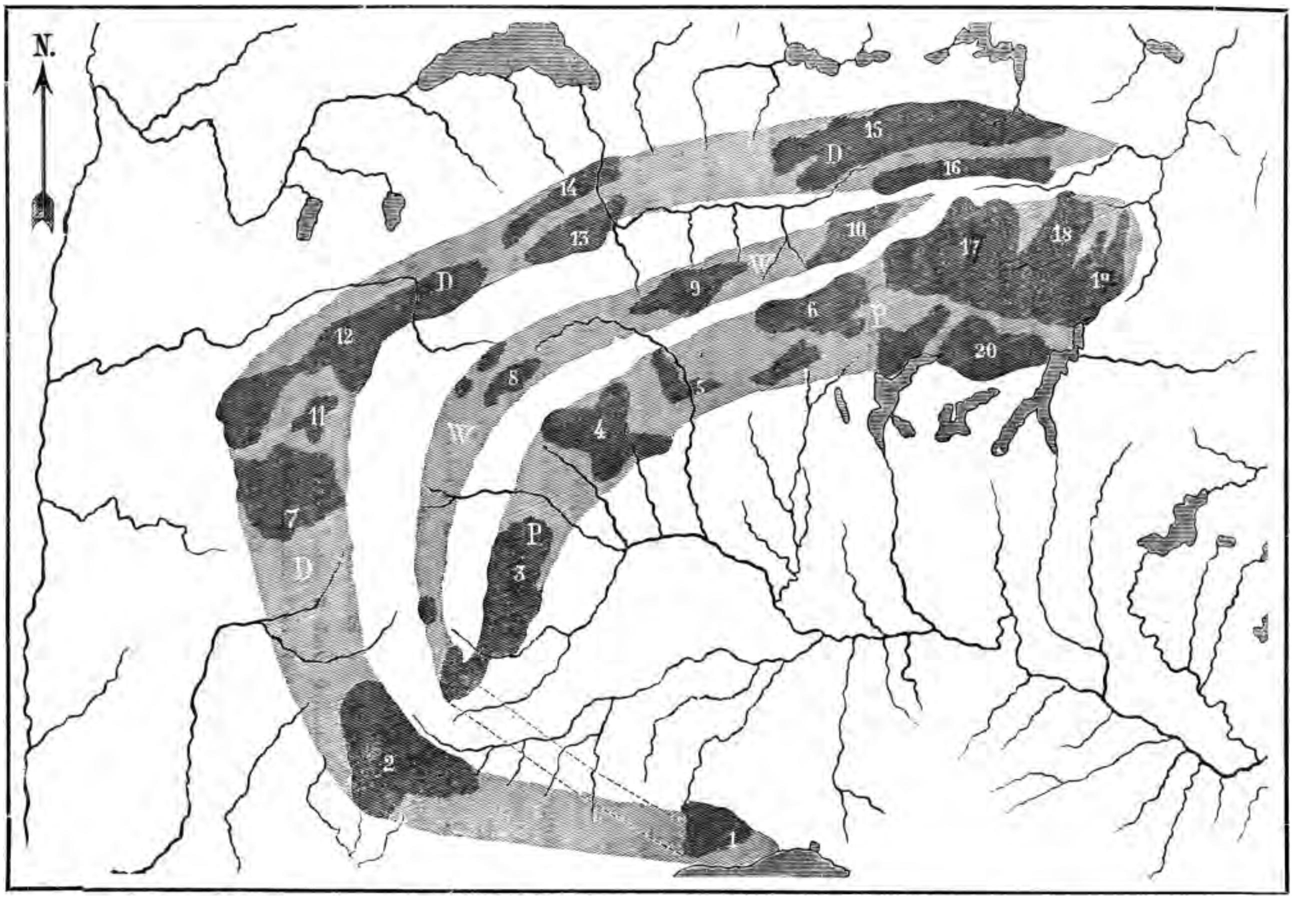

Figure 13: The structural outline of the Alps according to Desor (1865, fig. 5, p. 78). The grey areas are anticlinal ridges and the white areas are sedimentary rock-filled synclines. Desor regarded these as 'waves' as he had learnt from Henry Darwin Rogers. Key to lettering: D. Group or belt of Dauphiné, W. Group of Wallis, P. Group of Piemont. The numbers correpond with central massifs as follows: 1. Ligurian Alps, 2. Maritime Alps, 3. Cottian Alps, 4. Graian Alps, 5. Sesia, 6. Monte Rosa, 7. Pelvoux, 8. Vannoise, 9. Wallis, 10. Simplon, 11. Grandes Rousses, 12. Belledonne, 13. Montblanc, 14. Aiguilles Rouges, 15. Finsteraarhorn (now simply called the Aar Massif) , 16. St. Gotthard (now called Gotthard Massif), 17. Suretta, 18. Adula, 19. Ticino (actually this is the Bergell Massif; here Desor's hand must have slipped) and 20. should be Ticino.

[§] This is a slip of the hand of the great author: he meant to write Emmanuel. Emmanuel Marie Pierre Martin Jacquin de Margerie (1862-1953) was a friend of Suess and the principal translator and editor of the French edition of the *Antlitz*.

ness in the fifteenth century, i.e., it printed incunabula!) but the largest university press and is governed by its academics called the Delegates of the Press. It is also the publisher of the famous *Oxford English Dictionary* (OED; see Winchester, 2003, for the history of the origin of the OED and its surprising connexions with geology). For the most comprehensive history of this remarkable institution, see Eliot (2013). For a much shorter, more personal account covering the history of the Press mainly since 1860, see Sutcliffe (1978). In this book there is no meniton either of Sollas or Suess. Barker (1978) is a still shorter, illustrated history, but no substitute for either Sutcliffe (1978) or Eliot (2013). The third volume of Eliot (2013) deals with the period in which *Das Antlitz der Erde* was translated into English and published by the Press, but, as in Sutcliffe (1978), there is no mention of it in the *History*.

[2] William Johnson Sollas (1849-1936; Fig. 19) English geologist, zoologist and anthropologist. From 1897 he was the professor of geology at Oxford. Before that he had been lecturer in geology and zoology in the University College of Bristol and then professor of geology in the Trinity College, Dublin. He worked in diverse areas ranging from vertebrate and invertebrate palaeontology, through petrography to anthropology, although today he is widely remembered for his editing the translation of Suess' *Antlitz* into English by his daughter Hertha B. C. Sollas, while she worked as an unpaid research assistant. For a just assessment of Sollas, see Sarjeant, 1996, p. 78: 'Whatever his attainments and whatever his eccentricities, Sollas unquestionably held the chair too long; he was a poor teacher, undertook less and less research as the years passed, and allowed the Department to stagnate.' Professor Sollas was said to be a good linguist, however (see Vincent, 1994. p. 30). In his tribute to the work of the daughter and father, Professor 'David' Ewart Albert Vincent (1919-2012), one of Sollas' later successors in the Oxford Chair, wrote: 'It was a monumental achievement, and English-speaking geologists owed—and indeed still owe — a considerable debt of gratitude to both daughter and father for making The *Face of the Earth* readily available for their study.' (Vincent, 1994, p. 30). Hertha did a tolerably good job of her translation, although she was not alone at this work. From the Preface to vol. III (Suess, 1908), we learn the following:

'As an indication of the esteem felt for the veteran author of the '*Antlitz der Erde*,' the revision of the translator's rendering of this third volume has been undertaken by a number of geologists representative of all parts of the English-speaking world.

England is represented by Sir Archibald Geikie [1835-1924; at the time he had retired from the directorship of the Geological Survey of the United Kingdom and was a joint secretary of the Royal Society; only a year later he became ist president], the Rev. Prof. T. G. Bonney [1833-1923], Dr. J. H. Teall [1849-1924], Prof. Charles Lapworth [1842-1920; the man who introduced the Ordovician System], and Prof. W. W. Watts [1860-1947]; North America by T. C. Chamberlin [1843-1928; the co-author of the planetesimal hypothesis for origin of the earth]; India by R. D. Oldham [1858-1936; assistant superintendent of the Geological Survey of India; the discoverer of the earth's core; his father Thomas Oldham (1816-1878), a famous geologist, was the first superintendent of the Geological Survey of India and a friend of Suess, who visited Suess in 1862 in Vienna and obtained the services of Suess' student Ferdinand Stoliczka (1838-1874) for the Indian Survey, which marked the beginning of a fruitful collaboration between Suess' school and the Survey that was to last until the beginning of the twentieth century and culminated by Carl Ludolf Griesbach being made the Superintendent of the Survey]; South Africa by Dr. A. W. Rogers [1872-1946; author of the first geology of South Africa]; and Australia by Prof. T. W. Edgeworth David [1858-1934; knighted in 1920; the famous Antarctic explorer who first reached the south magnetic pole]. If it should seem that England itself plays too great a part in this list, a sufficient explanation will be found in the exigencies of time and space.

Notwithstanding the assistance I have thus received, I have not relaxed my own efforts as editor, and I have found it necessary to make frequent re-revisions in order to preserve some continuity and uniformity of style. Thus my responsibility remains unimpaired, and whatever improvement may be discerned in the translation of this volume must be attributed to the reviewers; any defect, on the other hand, to me. The task of turning German into English is a peculiarly difficult one, owing to the natural tendency of the English to revert to a Teutonic form.

The reverence due to a great classic has restrained us in this, as in previous volumes, from taking any liberties with the text, whether by comment or emendation. Our sole aim has been a faithful rendering. The extraneous matter of this preface would not have been inserted had it been possible otherwise to make known the tribute which the work of the revisers is intended to convey. W. J. S., Oxford, August, 1908).'

Despite all this work mentioned by Sollas, there are still numerous errors in translation. Many result from the translators' and editor's unfamiliarity of many of Suess' concepts and the areas he dealt with (goes to vindicate the late Bill Sarjeant's poor opinion of Sollas; he was clearly no de Margerie, who edited and in part carried out himself the superb French translation). No wonder Sollas felt compelled to ask Suess to send him some sketches to elucidate some of the particularly difficult concepts. Clearly, they were helpful, spectacular witnesses of how much Suess' interpretations were closer to ours now than those of many of his successors in the twentieth

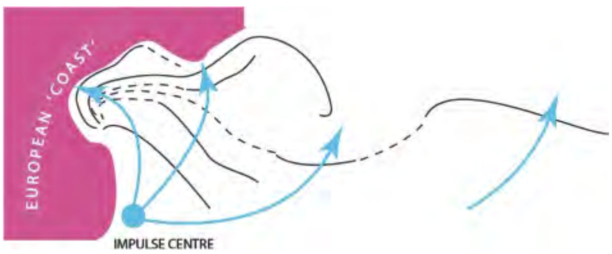

FIGURE 14: Suess' wave simile for the mountain ranges he discussed in Suess (1873a).

century, but still not sufficient for a more thorough understanding of the overall message of the *Antlitz*. For all those who can read the original German, I would recommend sticking to it, but also consulting the additional figures and the literature references in the French translation and the additional maps in the Spanish translation. I would also recommend reading the abstracts of the individual volumes that the Spanish translator Pedro de Novo y Chicharro placed at the front of each volume. In 1920, Pedro de Novo y Chicharro also published a booklet introducing Suess' book to the Spanish reader, in which he relates Suess' predecessors, the importance of Suess' book, the difficulty of getting to Suess' theories and the relevance of the *Antlitz* to the geology of the Iberian Peninsula (de Novo y Chicharro, 1920). It is a most laudable little book for those who wish to understand the *Antlitz*. In the quotations from Suess, I have used Sollas' English version, but not infrequently had to make corrections to the translation.

[3] This is the clearest statement by Suess anywhere of the aim of not only of the *Antlitz*, but of his own tectonic studies and his philosophy of science. He was not after 'laws,' which he knew would be not laws but just hypotheses, but if formulated as laws, as some of his successors in the twentieth century attempted as indicated above, they might have solidified into dogmas. He wanted to avoid such a course and only wished to see certain common patterns in the record the tectonic phenomena leaves behind and hoped to establish connexions and correspondences between them so as to understand the message written on the face of the earth by them. This was a part of his critical rationalist approach and what I elsewhere called the 'comparative-judgemental' method he developed in global tectonics. A beautiful example of the application and the success of this method is illustrated by Suess' description in *Die Entstehung der Alpen*, of the western Tien Shan as a south vergent chain (Suess, 1875, pp. 140-141; for his sources, see his note 173 on p. 167) on the basis of the descriptions of von Semenow (1858) and Sewerzoff (1875). The geology is mainly from von Semenow (1858), who draws the chains as straight lines (von Semenow's plate 16) following the custom of the day and simply indicates that the igneous rocks sit mainly in the north. Sewerzow provides a much better geographical map (see his coloured foldout map). Only on the basis of his experience mainly from the European mountains and from his reading of the then only slightly better-known western Himalaya, Suess deduced that the straight chains of Semenov were actually south convex arcs and this, combined with the position of the igneous rocks, indicated a south-vergent chain. All of this was later corroborated by the great Russian geologist Ivan Vasiliyevich Mushketov's (1850-1902) letter of 1881 and the manuscript of his *Turkestan*[††] which Suess received before writing the second part of the first volume of the *Antlitz* (see Suess, 1885, pp. 598-603, also see note 2 on p. 650).

[4] In the *Antlitz*, wherever the term 'Archaean' is used, it means all crystalline, non-fossiliferous rocks below the Palaeozoic rocks. Today it is customary to refer to that part of earth history as Precambrian or Prephanerozoic. The term Archaean was introduced by Dana (1872, p. 253) in the following context and with the following words:

'Archæan rocks.— Besides the limestone and Taconic schists and gneiss, there is, near Poughquag, in still more intimate con-

Figure 15: The mountain ranges and fold bundles in the Alpine foreland according to Suess' 1873a description. I have attached to them arbitrary rigid blocks. Suess mentions such blocks as we have seen, but does not specify their boundaries except where their motion leads to shortening and where to extension. Therefore the boundaries sketched in this figure are arbitrary except for those.

Figure 16: The motion of the blocks Suess deduced mainly from their shortening frontal parts. Notice that the shortening he deduced in the Alpine foreland requires extension in front of the Alps, which, however, is compensated by Alpine shortening (by the wholesale northern motion of the Alps and the northern Carpathians: Suess, 1868). But the main message of this and the preceding figure is to emphasise the principle: independent motion of rigid to semi-rigid blocks separated by narrow zones of deformation. This is essentially plate tectonics without subduction. This is what Suess compared with the drifting of pack ice (Suess, 1875, p. 156).

** In North America, the word 'judgemental' in English acquired a negative connotation, because of the prevailing 'politically correct' atmosphere. Judging others is considered something undesirable. This, of course, is an ideal recipe to kill criticism and therefore the scientific thinking. Fortunately, when Suess was alive, being judgemental was considered a very positive attribute. This is what he means when he writes 'A criticism of previous views must in addition be the natural result of this method.'

†† Regrettably, Mushketov's early death hindered the completion of this great book. For the completed parts, see Mushketov (1886, 1906), which I received as a precious gift from the late Alexei Borisovich Natal'in (1919-2003) during a visit in Tashkent in 1995. The manuscript Suess used belongs to the first volume.

nection with the quartzite, rocks of the Azoic age, a continuation of the Highland range of New Jersey— a range recognized as Azoic first by H. D. Rogers, and shown to continue into Dutchess county by Logan and Hall (this Journal [by which he means the *American Journal of Science*], II, xxxix, 96). They are probably Laurentian, as stated by Logan and Hall, that is, they are equivalents of the oldest known Azoic rocks of Canada. But as this point is not definitely settled, and since the term Azoic has been ruled out by facts that the era was not throughout destitude of life, I propose to use for the Azoic era and its rocks the general term Archæan (or Arche'an) from the Greek ἀρχαιος , pertaining to the beginning.'

Here Dana added the following footnote:

'Whatever part of the Archæan beds are proved to belong to an era in which there was life, will be appropriately styled Archeozoic. This term avoids the objection which Eozoic derives from the doubtful nature of the Eozoum.'

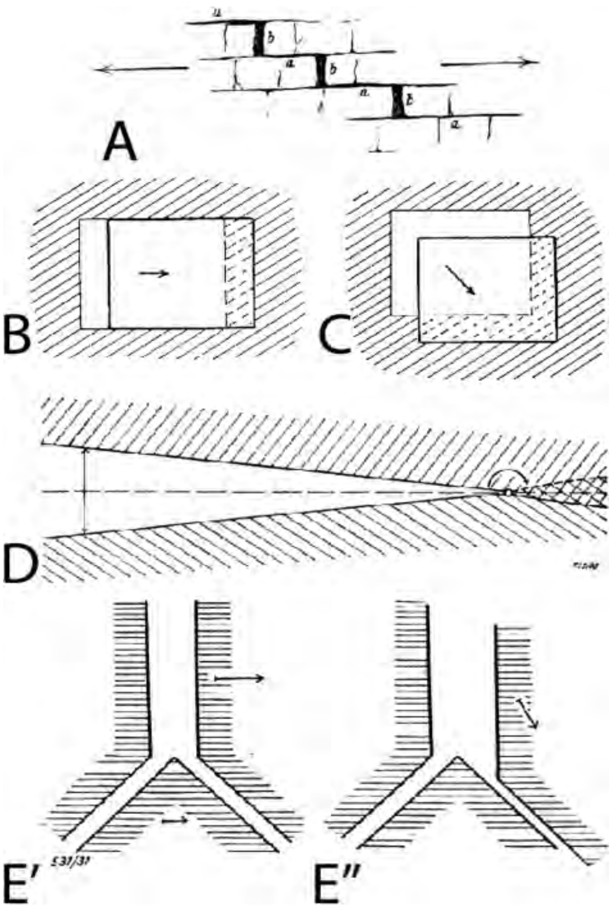

Figure 17: Franz Lotze's examples of block motion in the Alpine foreland published in his 1937 paper. What Lotze says is identical to what Suess says, but Lotze was evidently unaware of Suess' abstract. It is, however, less forgivable that he also must have remained ignorant of *Die Entstehung der Alpen*. Possibly his teacher Hans Stille, one of the chief architects of the Dark Intermezzo, told him that Suess was *passé*. A. Extension (along b's) with small 'transform faults' (a segments). B. Block motion creating extension on one side and convergence on the other (this is what Suess was talking about in 1873a). C. Oblique motion giving rise to oblique displacements. D. Pivot poit between two blocks rotating with respect to one another, creating extension on one side of the pivot and shortening on the other. Wilson (1965) was to call such pivots 'transforms' (see Fig. 18.) E' A triple junction formed by the direction of motion indicated by the arrows. E" The same triple junction after the rightmost block has changed its motion from pure extension with respect to the other two blocks to dextral transtension.

Dana later discussed the Archaean in detail in the second edition of his *Manual of Geology* (Dana, 1875, pp. 146-161), which we know for sure that Suess read (from his references to it in *Die Entstehung der Alpen*: Suess, 1875).

The 'Archaean' mentioned by Suess is now known to be Hercynian metamorphic and igneous rocks (for a current geological map of the surroundings of Karlsbad, see Blecha and Štemprok, 2012, fig. 2). Suess encountered them while staying in Karlsbad (now Karlovy Vary in the Czech Republic) in 1850 and was asked to contribute a 'geognostical' section to a guide to be published by a local publisher:

'Karlsbad, the valley cut into granite, offered such a contrast in its landscape as in its structure to Prague, as well as to Vienna, that I did not tire of wandering and observing with attention. This was noticed and the Kronberg bookshop invited me to write a geological, or, as it was then called, geognostic, section for a guide for the guests of the spa which it was about to publish. This was my first publication. The booklet appeared in the winter of 1850/1851.' (Suess, 1916, p. 74).

His contribution appeared anonymously (see Anonymous, 1851, pp. 39-47; the guide proved popular and was continuously updated appearing in numerous editions, in which Suess' section appears unchanged save for a few alterations in the wording.

[5] I do not know what the 'Silurian' here refers to: is it to the Silurian *sensu stricto* as it is exposed around Prague (Chlupáč et al., 1998, see the coloured geological map inside the front cover) and not to the entire early Palaeozoic, i.e., to the Barrandian as a whole, according to the graptolites Suess (1851a, b) describes; or does it include the entire Barrandian from the Cambrian to the Devonian (Chlupáč et al., 1998)? Because Suess mentions here also the 'Archaean', it might be that he even wandered far from Prague into the metamorphic rocks of what was later to be called the Bohemian Massif apart from those he saw around Karlsbad. Suess, in his later writings, used the Silurian in its Murchisonian sense, i. e., for the entire Lower Palaeozoic. He did, however, use the Cambrian from 1901 onwards (Suess, 1901: 'pre-Cambrian rocks', p. 16; 'Cambrian table', p. 394 etc.), but not the Ordovician.

Well into the last quarter of the nineteenth century Silurian was commonly used as the lowest system of the Palaeozoic, below the Devonian. Let us remember that Charles Lapworth (1842-1920) proposed the Ordovician already in 1879 to resolve the ongoing Cambrian-Silurian controversy (Secord, 1986). It took some time for the Ordovician to be accepted by the international community, mostly because of the resistance of Murchison's associates and successors in the Geological Survey of Great Britain (see Secord, 1986, pp. 304-306; the Survey recognised the Ordovician only as late as 1901 and only after the retirement of Sir Archibald Geikie, Murchison's protégée: Secord, 1986, p. 310). For example, in the last edition, prepared by himself, of his much-celebrated textbook

Géologie Stratigraphique (Gignoux, 1950; the great Alpine geologist Rudolf Trümpy named it among the 'brilliant syntheses' of Alpine geology: Trümpy, 1960, p. 882; I do not think it was) the famous French geologist Maurice Gignoux (1881-1955) did not use the Ordovician at all and this was not considered worthy of notice by some of his continental reviewers of great repute (see, e.g., Buxtorf, 1950). His book was translated into English in 1955 under the title *Stratigraphic Geology* and one of its British reviewers simply pointed out that Gignoux had combined the Ordovician and the Silurian into one, while hailing the book as 'a much needed and unparalleled work of reference' (R. G. C. B., 1956). In fact, beginning with the second edition of his book (Gignoux, 1936), Gignoux added the following footnote to his chapter on the Silurian (p. 77, footnote 1; in the last edition: Gignoux, 1950, p. 80, footnote 1; in the English translation: Gignoux, 1955, p. 99, footnote 2): 'At the present time, many British geologists prefer not to use the term Gothlandian, which they deem not precise enough. They replace it by that of Silurian, used in a restricted sense. They distinguish, thus, in the lower part of the Primary [he means Palaeozoic], the three systems, Cambrian, Ordovician, and Silurian.' However, not only the British, but also the American and some German authors had already begun using the Ordovician already in the first half of the twentieth century, Americans having done so considerably earlier (when Suess was alive and active: e.g., Walcott, 1903, p. 25; Chamberlin and Salisbury, 1904, p. 18; Grabau, 1921, p. 256; see also Grabau's useful historical review of the development of the geological time scale between his pp. 2 and 20) than the Germans (e.g., Emmanuel Kayser never used the Ordovician in his extremely successful and widely used textbook, the last, 7th, edition of which having come out in 1923; this situation changed later: e.g., von Bubnoff, 1941, p. 108). In Austria, none of the textbooks in the first quarter of the twentieth century used the Ordovician (e.g., Toula, 1900; last edition: 1918; Kober, 1923; Schaffer, 1924). I cite all these publications just to show how slowly the stratigraphic terminology stabilised. There were even individual suggestions as late as 1933 to add other systems (for a good history of some of the North American attempts, see Weiss and Yochelson, 2012): Field (1933, p. 22), for example, added the Ozarkian (first defined as a time-rock term by Broadhead, 1891) and Canadian (first defined by Dana in 1874; now an informal North American system of the Lower Ordovician) between the Cambrian and the Ordovician in his textbook, following Ulrich (1911) and inserted a Comanchean (now used informally for the Lower Cretaceous) between the Jurassic and the Cretaceous, although these suggestions were not accepted. Neither was Charles Schuchert's division of his Palaeozoic into Georgic, Acadic, Ozarkic or Cambric, Canadic, Ordovicic and Cincinnatic to be followed by the 'Neopalaeozoic' Silurian (Schuchert, 1910)! This sort of instability was probably why Suess was fairly relaxed about the terminology he used. He has never been a great enthusiast about terms and definitions, anyway. It is interesting that while the translation of the first part of the third volume of the *Antlitz* was being revised, Lapworth, who was one of the revisers, made no objection to Suess' omission of his term Ordovician probably ouf of 'reverence due to a great classic' as Sollas wrote (see above, endnote 2 to this section).

[6] Around Prague and Karlovy Vary in the Czech Republic.

[7] The famous *Dachsteinkalk*, from Norian to Rhaetian. In 1851, Suess had become a volunteer in the service of the Imperial and Royal Geological Survey in Vienna and in that capacity he was detailed as an assitant to Franz von Hauer (1822-1899), whose task it was to construct a geological cross-section across the Austrian Alps from Passau in the north to Duino in the south. Suess had asked for the highest part of the geotraverse to map, which was the great Dachstein plateau. His general results were incorporated, under his own authorship, into von Hauer's paper (Suess, 1857). He published the brachiopods of the Kössen beds from the area separately (Suess, 1854).

Another fruit of this study was Suess' discovery that in the Mesozoic deposits, from the Triassic Dachstein Limestone all the way up into the Gosau conglomerates of Cretaceous age, clasts of older metamorphic rocks of the underlying Palaeozoic units were found. Suess found this observation so unexpected that he returned to the area in the autumn of 1859 with his students Ferdinand Stoliczka and Edmund von Mojsisovics Edler von Mojsvár (1839-1907). This reexamination showed that the so-called central crystalline massifs could not have been younger than the overlying Mesozoic and Cainozoic sedimentary rocks and they therefore could not have caused their deformation as believed at the time. In a later paper describing these observations (Suess, 1860a), Suess makes no remarks concerning the significance of his discovery for the cause of the Alpine deformation, but it no doubt showed him that all was not well with the then reigning theory of primary

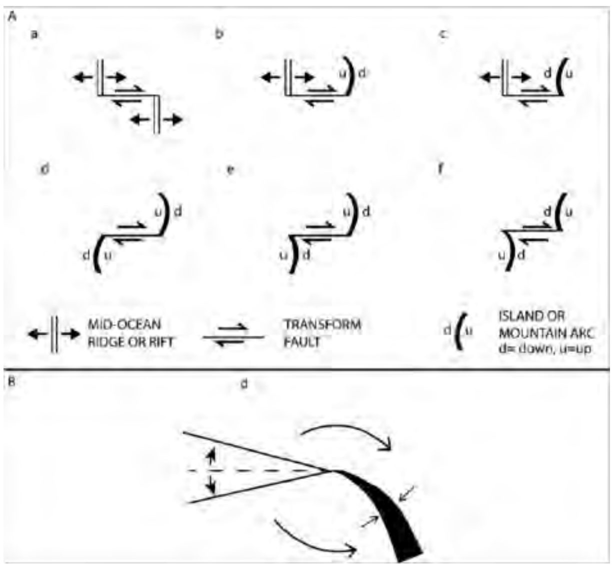

Figure 18: Wilson's transform faults (a) and transform point (b). Compare these figures with Lotze's figures in Fig. 17 and with the Figures 15 and 16 drawn after Suess' descriptions in Suess (1873a) and consider them in the framework of his comparison with drifting pack ice.

uplift by means of 'magmatic intrusions' represented by the central crystalline massifs.

Suess does not mention this discovery in his later publications as a support for the passive behaviour of the central massifs during the Alpine deformation, but, using many other observations, both from the literature and from among his own, he stresses their older age than the Mesozoic deposits of the Alps and their passive behaviour during the Alpine mountain-building (e.g. Suess, 1875, pp. 8-13).

[8] This is the Elbsandstein. Cenomanian to Santonian mostly grey to buff shallow marine quartz sandstones, with 10 to 30% porosity and weathering into ashlar-like masses (hence the name *Quader*=cuboid) because of the horizontal bedding and vertical jointing. A strange pseudokarst with lapies forms and small caves has developed in it because of increased Fe_2O_3/K_2O from surrounding igneous rocks (see Labus and Labus, 2011). It is widely used as a building stone.

[9] The key to the difference is in the three following statements by Suess (freely translated by me). First, the *structural* difference:

'The most correct way to picture the Austrian Limestone Alps is as a broad and mighty strip of limestones resting on red sandstones and shales (the Werfen shales). It not only displays under its hogbacks, turned toward the central stock, a continuous strip of these shales, but it is also disrupted along many anticlinal lines that are parallel with one another also exposing the Werfen beds underlying it. In this way the limestone massif is divided into a number of geotectonic[‡‡] units or individual parts, which, on a map, appear to be surrounded by Werfen beds. These anticlinal lines do not always correspond to the lowest parts of the valleys, however.' (Suess, 1857, pp. 51-52).

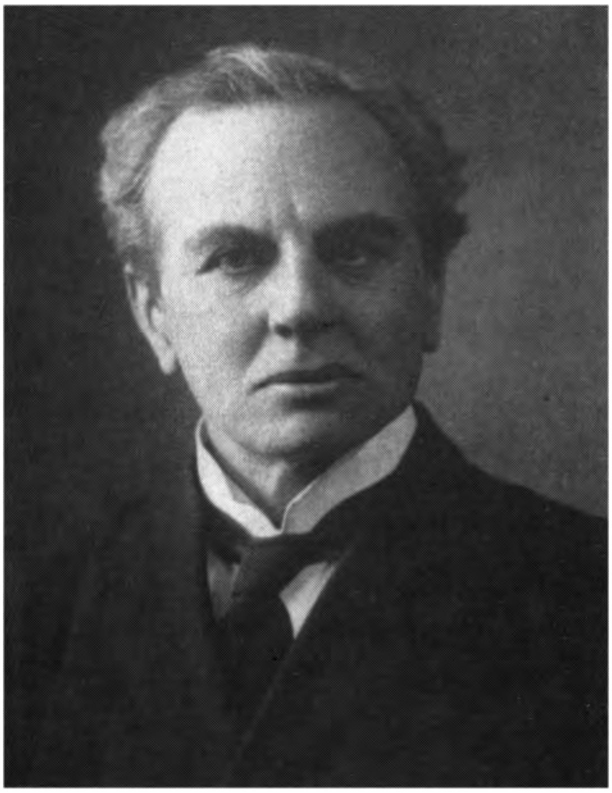

Figure 19: William Johnson Sollas.

Here what must have struck him is the long, parallel anticlines of the Northern Calcareous Alps, as opposed to the rounded outcrop pattern on the Bohemian Massif resulting from the presence of granitic and gneissic coupolas and the flat-lying Cretaceous cover. This difference is the first thing Suess emphasises in *Das Antlitz der Erde*, as soon as he begins to discuss the structure of the earth's surface. He writes:

'The variety of outline in the mountain masses forming the northern foreland of the Alps and of the Carpathians stands in striking contrast to the uniformity of the long and gentle curve which marks the northern border of the Alpine chain.' (Suess, 1883, p. 285).

The second statement about the difference between the Alps and their foreland concerns the disparity *in the fossils* in the foreland and in the Alps, which Suess first pointed out in his monograph on the brachiopods of the Kössen Beds, based on his study of the Dachstein area:

'The geological characteristics of our Alps, especially the palaeontological ones, are little known to the general public. What is here called "lower Lias" is in many aspects different from that, which is so called in England or in Swabia.' (Suess, 1854, p. 29).

This 'palaeontological difference' has long bothered Suess. He repeatedly stressed it in many of his writings, which finally led him and his associates and students, such as Edmund Mojsisovics Edler von Mojsvár, Theodor Fuchs (1842-1925), Melchior Neumayr (1845-1890) and Carl Diener (1862-1928), four of the Viennese giants, to palaeobiogeographic studies. Concepts such as Tethys and Gondwana-Land are some of the most widely-known fruits of these studies.

The same palaeontological difference between the Alps and their foreland was also the reason why the foreland stratigraphic sequences could not be correlated with the Alpine sequences for the longest time. A giant step toward the solution of this problem was taken by Suess and the great Swabian palaeontologist and stratigrapher, the creator of the concept of 'biostratigraphic zone,' Albert Oppel, when they were able to correlate the *Avicula contorta* (today revised as *Rhaetavicula contorta* PORTLOCK (1843)) beds (containing also *Cardium Rhæticum* MERIAN, today revised as *Protocardium rhaeticum* (MERIAN, 1853) and *Pecten Valoniensis*; today revised as *Praechlamys valoniensis* DEFRANCE (1925))[§§], around Stuttgart with those in the Dachstein and thus to provide one reliable reference point for the troublesome Triassic correlations between the foreland and the mountains (Oppel and Suess, 1856). This was such an important thing at the time, that Sir

[‡‡] The usage of this term strongly suggests that Suess by that time had read the first volume of Naumann's *Lehrbuch der Geognosie*, on p. 899 of which the term *Geotektonik* had made its first appearance (Naumann, 1850).

[§§] Formerly, the specific names of organisms were also capitalised if they were taken from a proper noun. This practice is now abandoned.

Charles Lyell specifically asked Suess to send him a summary of the new discoveries concerning the Triassic of the Alps to be included in a supplement to the fifth edition of his *Manual of Elementary Geology* (Lyell, 1855). The supplement had two English editions (Lyell, 1857a and b) and one French translation (Lyell, 1857c). There was no German translation, because its contents were incorporated into the German translation of the fifth edition (Lyell, 1858).

The third difference between the foreland sequences and those in the Alps pertained to *stratigraphic facies*. Suess knew that the foreland Mesozoic facies was usually representative of restricted environments very close to land and commonly received land-derived debris. He did not think this was case in the Alps. He wrote (freely translated by me):

'From the considerable thickness of the calcareous masses, from their purity and from their abrupt cessation one could very well be sure that they were deposited far away from the shore.' (Suess, 1857, p. 313).

Hsü (1973, p. 67) took exception to the following statement by Suess in *Die Entstehung der Alpen*, which is based on Suess' observations in the Dachstein that I cited above and later in other localities in the Northern Calcareous Alps and was one expression of his view of the differences between the Alps and their foreland. Suess had written:

'When someone compares the Western Alps with the French Central Plateau or the Eastern Alps with Bohemia, the greater completeness of the sedimentary series in the Alps and their lacunar development in both other regions immediately attract the attention. This is striking especially in the second case. Our Eastern Alps possess not only very complete and very thick sedimentary packages, but where their equivalents can be followed towards, for example, Swabia or Franconia, the Alpine occurrences are without exception of a more pelagic character. This is seen, for example, in an excellent manner in the Rhaetian Stage, the individual deposits of which I can only see as the deep parts of one and the same sea.' (Suess, 1875, pp. 97-98).

Hsü objected: 'The European geologists, particularly the Austrian and French masters who studied the Alps before the turn of the century, took issue with Hall [meant here is the American palaeontologist and stratigrapher James Hall (1811-1898) the reviver of the geosyncline idea in North America]. Suess first advocated the idea that the geosynclinal sediments represent a pelagic facies. His arguments were, however, rather tenuous. He was impressed by the fact that geosynclinal sequences are not only thicker, but also more complete and are devoid of unconformities, as compared to their shelf equivalents. He was wrong, however, to cite the Triassic of the eastern Alps, known to us as a tidal-flat complex (Fisher, 1963 [*sic*!]***), to prove his point that the Alpine carbonates are largely pelagic. Equally erroneous is his interpretation that Mesozoic benthonic faunas represented relic Palaeozoic forms that had survived in oceanic deeps. Suess was evidently not a sedimentologist.' (Hsü, 1973, p. 67).

Unfortunately there are several serious errors in Hsü's statements, both concerning the history of ideas and the geology of the limestone sequences in the Eastern Alps and a critical ommission. The first error is his statement that Suess was the first one to advocate that geosynclinal sediments represent a pelagic facies. The idea of a preparatory trough before mountain-building sets in, filled with sediments, mostly pelagic in environment and coincident with the future lie of a mountain-range, has been explicit in the writings of Élie de Beaumont since 1828, i.e., before Suess was even born (the geosyncline was therefore *not* an American innovation by James Hall, as Hsü claims on his p. 66; on this issue see Şengör, 2003 and Schaer and Şengör, 2008):

'... I consider as obvious that this thickness [of the beds near Petit Cœur in the French Alps, along the western margin of the Belledonne Massif] (composed of the sum of the thicknesses of all the intermediate beds measured perpendicular to the planes of stratification) is no less than two thousand metres. Now, in those parts of Europe where one normally looks for this type of formations, there is none that approaches a similar thickness, not even any that shows mineralogical characters exactly comparable with those deposits of which we are speaking. These differences of composition are perhaps a neces-

Figure 20: Bernhard Studer.

*** This is probably a misprint. The correct date is 1964.

sary consequence of the enormous difference of the thickness that I just mentioned; and these two kinds of differences combine with some other considerations indicated above to make me think that the system of beds, with which I deal in this note, were deposited in a very deep sea, while the most-studied parts of the Jurassic deposits were laid down in shores, where they were crowned, at intervals, by coral reefs. The central part of the Alps seem to offer to our regards a pelagic state of deposition, whereas the deposits of the ridges around Bath and Oxford present us a littoral state.' (Élie de Beaumont, 1828, pp. 376-377).

There is nothing objectionable in what Élie de Beaumont wrote with remarkable insight, especially when one considers that already in those days 'pelagic' rightly did not mean deep water, but away from the influence of land, following Lavoisier's remarkable paper on marine facies (Lavoisier, 1789, plate III). Now let us go to the Eastern Alps: in the Dachstein, as one goes northward (at least in the present setting of things; the actual palaeogeography of the Hallstatt/Dachsteinkalk is still controversial), one plunges into the Hallstatt trough, in which, the Upper Triassic sediments clearly show not only a frankly pelagic, but also deep-sea facies. Around it, the facies is shallow, but still far away from a shore. Recall that Suess does not say 'pelagic', but 'more pelagic.' Fisher (1964, p. 144), whom Hsü quotes, ascribed the Lofer cyclicity seen in the Dachstein Limestone to changing sea-levels. He wrote: ' Assuming a regular oscillation pattern, one can set up three extreme cases ... One in which the area was generally high and dry, but was periodically submerged for a brief interval; one in which periods of emergence and submergence of about equal duration alternated; and one in which a normally submerged condition was periodically interrupted by a brief interval of emergence. Of these alternatives the last appears the most likely.'

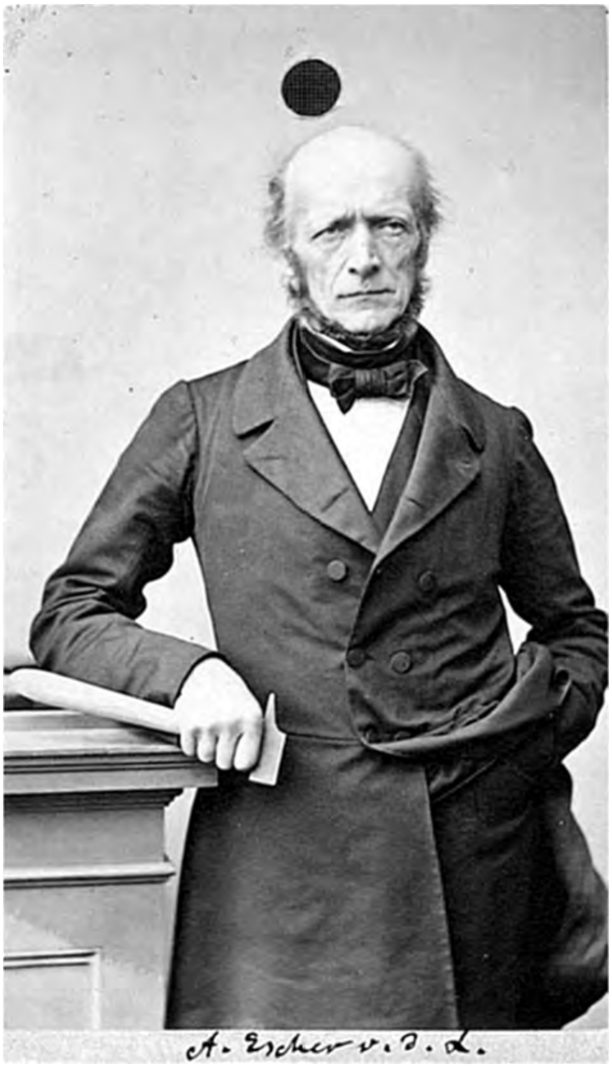

Figure 21: Arnold Escher von der Linth.

The area that Suess calls 'pelagic' thus had both deep regions and shallow regions, both far away from any continental shore (although clearly there were local shores of reefs in the middle of the open sea, like the reefs seen on the Sahul Shelf today in the Malay Archipelago, sitting entirely on continental crust, in the middle of high seas, just as in the Alpine case). Moreover, we know that Suess knew about the shallow-water areas near coral reefs within the Starhemberg Beds of the Dachstein (Suess, 1854, p. 32; p. 4 of the offprint), which Hsü ommited to say, probably because he did not appreciate the fact that Suess in those days had considered the Kössen and the Starhemberg Beds as belonging to the Lias. Therefore, notwithstanding Hsü's objection, Suess' summary thus stands largely correct; he only erred in following Élie de Beaumont by equating thickness and freedom from unconformities with great water depth in general. He later pointed out, using the Challenger reports, however, that in places the depth of the Sea in the Alps may have exceeded 4000 m in the Triassic. In that he was probably right for the Hallstatt facies. He was perhaps not as bad a sedimentologist as Hsü believed, despite the fact that many of the concepts, which Hsü unhistorically used to condemn Suess, had not yet been developed.

The Bohemia/Alps contrast made a very deep impression on Suess' mind. He not only greatly elaborated on it in the *Antlitz*, but returned to it even his farewell lecture in 1901:

'Soon, however, it was recognised that in the Bohemian Massif the stratigraphic sequence was far less complete than in the adjoining regions of the Alps, and that in Bohemia particularly there is an extraordinary interruption of marine deposits extending upward into the Middle Cretaceous, whereas in the Alps all these great epochs are represented by marine strata. This same transgression of the Middle and Upper Cretaceous shows up again in Galicia, then far into Russia, on the other side of the French Central Plateau, on the Spanish Meseta, in large parts of the Sahara, in the valley of the Mississippi, and northward over this region to the vicinity of the Arctic Sea, in Brazil, finally on the shores of central and southern Africa, in east India; and, in fact over such extraordinarily vast regions that it became impossible to explain such transgressions of the sea, according to the older views of Lyell, by means of the elevation and depression of continents any longer.

....

Meanwhile, more and more light came regarding the strange development which certain Mesozoic deposits, particularly the Triassic of the Alps, show when compared to the north-lying lands, as Württemberg or Franconia. The observations in Asiatic highlands, especially in the Himalayas, taught that this type of Triassic development has a very wide distribution toward the east; and it even became possible to prove that directly across Asia, from the existing European Mediterranean to the Sunda Islands, there once extended a continuous sea. This sea has, as you know, received the name Tethys. The old continent along its southern side was named Gondwana-Land, and that on its northern side, Angara-Land. The present Mediterranean is a remnant of the Tethys.' (Suess, 1901, pp 3. and 4; I use here Charles Schuchert's translation into English in Suess, 1904b, with only slight changes and correction of what seem to be typos).

His Alpine studies thus corroborated his earlier hunch that global stratigraphy was not controlled by continental up and down motions (cf. Suess, 1916, p. 139), but by the movements of the sea-level, which he was to call eustatic movements in the *Antlitz* (Suess, 1888, p. 680).

Finally let me quote his student Theordor Fuchs' statement about the origin of Suess' comparative regional geological studies. He claims that the great contrast between Bohemia, the geology of which resembled that of areas north of the Alps in western Europe, and the Alps, the geology of which had no resemblance whatever to those regions and the desire to understand why that was so was what pushed Suess to his comparative studies:

'It was the profound contrast between the Alpine and extra-Alpine regions in western Europe that led Suess onto the way of comparative consideration of geological conditions and inspired him for those studies that found their temporary conclusion in the "Structure of the Alps" [here Fuchs means *Die Entstehung der Alpen*]' (Fuchs, 1909a, p. 21).

[10] Bernhard Studer (1794-1887; Fig. 20) Swiss geologist and physical geographer. Studer was one of the two founding fathers of what one might term the intermediate phase in the geoloy of Switzerland, from the time of the establishment of biostratigraphy in 1796 to the discovery of large nappes in 1884. The other founding father was his colleague and friend Arnold Escher von der Linth. Studer was responsible for many of the discoveries that eventually led to his book, *Geologie der Schweiz* in two volumes (1851 and 1853). It was this book which gave Suess his initial overall view of the Alps, which he used in his own 1862 book entitled *Der Boden der Stadt Wien nach seiner Bildungsweise, Beschaffenheit und seinen Beziehungen zum Bürgerlichen Leben*. Suess ended up rejecting its main thesis, however, namely the primary uplift theory. In Suess' development of his idea of the importance of horizontal motions in the Alps, a small paper by Studer, Les *Couches en forme de C dans les Alpes* (Beds in the form of a C in the Alps: Studer, 1861), played an important rôle (see Suess, 1875, pp. 16, note 23; p. 49, note 43).

[11] Arnold Escher von der Linth (1897-1872; Fig. 21), Swiss geologist and physical geographer. Escher is one of the towering figures, not only in the history of geology in Switzerland, but in the whole world. Although he published little, his amazing ability to observe and to understand rocks and geological relationships in the field and his unparallelled generosity to share his observations and ideas with others gained him many friends in and outside Switzerland. His greatest claim to fame was his discovery of the relationship in Glarus that later led to the discovery of vast overthrust nappes by Marcel Bertrand. Escher was a peculiarly diffident man when it came to publishing his observations and ideas (he did not even note down any ideas that might have occured to him during his observations with the fear that those ideas might mislead his observations! At least that is the reason his student Albert Heim gives as to why there are no ideas noted in Escher's meticulously kept field note-books: Heim, 1929, p. 216) and was truly shocked by his own discovery in Glarus. He showed it to Murchison, but begged him not to publish it with the fear that people might think him mad (see Trümpy, 1983 and 1991). Escher also showed it to Suess in 1854 and explained to him his theory of the famous double fold that minimised the implied magnitude of horizontal motion. Suess listened respectfully, disagreed, but said nothing.

[12] For the history of the invention of the idea of the double fold of Glarus, see Heim (1929), Trümpy (1983, 1988, but see 1991 for his note, on the basis of a personal communication from me, that Suess had noted that already in 1854 Escher had developed his interpretation), Westermann (2009) and Letsch (2011, 2014).

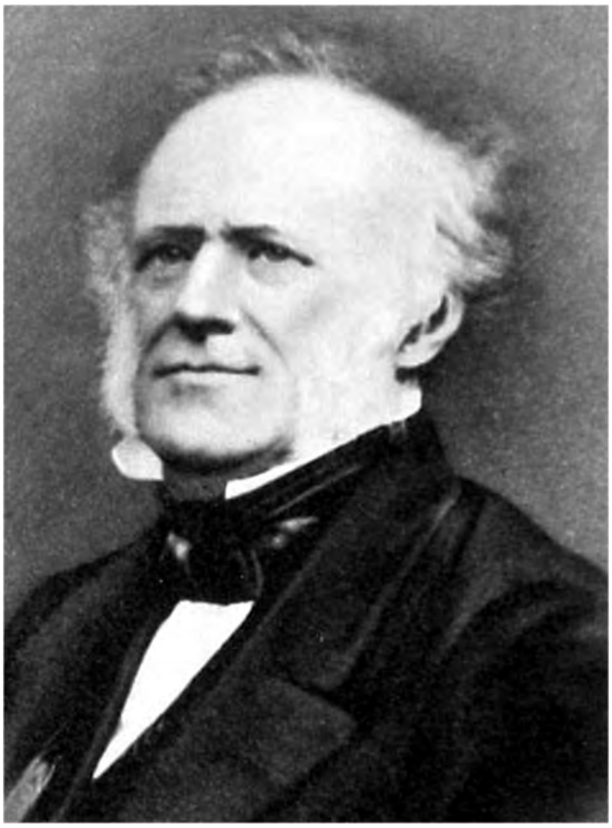

Figure 22: Sir Charles Lyell in the years when Suess first met him.

[13] Suess' meeting with Studer and Escher occurred during the convention of the General Swiss Society of the Entire Natural Sciences (*Allgemeine Schweizerische Gesellschaft für die Gesammten Naturwissenschaften*) in St.Gallen. The meeting took place on 24th, 25th and 26th July 1854. In his memoirs, Suess wrote the following concerning this event:

'In the summer of 1854 I travelled in the company of F. v. Hauer to St. Gallen to attend the meeting of the Swiss natural scientists. This was the first attempt to bring into contact the geological studies in the Eastern Alps with the more advanced ones of the west and the contact was such a happy one that two of the great geologists of Switzerland, the old member of the Council of Basel, Peter Merian [1795-1883] and Arnold Escher von der Linth offered to accompany me on my travel back home on foot all the way to Innsbruck. This was for me a most enjoyable trip.' (Suess, 1916, p. 103).

Klemun (2009, p. 317) published a most important letter by Suess to Escher showing that the initiative for this trip came from Suess himself, who also asked Escher to invite von Hauer. This is quite extraordinary, because at the time Suess was only 23 and only an assisstant in the İmperial Mineral Cabinet (the ancestor of the present Natural History Museum in Vienna), whereas von Hauer was ten years older and already the second geologist of the Imperial and Royal Geological Survey.

Suess reported on the tectonic discussions of the return trip with the following words:

'In 1854, at the time of my first visit to Switzerland, there existed a difference of opinion between the two important Swiss geologists Bernhard Studer and Arnold Escher von der Linth. It was about the structure of the high mountains of the cantons of Glarus, St. Gallen and Appenzell. Escher explained his concept to me from the height of the Säntis.

One sees in the lower parts of the valleys of the Linth and the Sernf all the way up to the glaciers of the Clariden a friable, young formation consisting of shales and sandstones, the flysch. In the valleys mentioned, this is overlain by older beds in wide areas. Studer recognised the fact, but did not dare to offer an explanation. The more courageous Escher thought to be able to recognise in the overlying mountains two halves or, as he expressed it, wings. The southern wing would occupy the space from the Lower Rhine valley towards the north all the way to the northern slopes of the high mountains coming from northeast of Tödi (Vorab 3025 m, Piz Segnes 3120 m, separated from it Ringelspitz 3206 m). North of these slopes, one reaches their underlying flysch of the already named valleys and north of the flysch the south-facing edge of the northern wing (Kärpfstock 2798 m, Ruche 2613 m, Grey Horns towards Pfäffers 2817 m). Escher's hypothesis was that the mighty southern wing moved from the south and the no less mighty northern wing from the north onto the flysch.

This was the tectonic problem which, under the name of the Glarus noose or the Glarus double fold, occupied geologists for many years.' (Suess, 1916, pp. 422-423).

Suess thus heard things from Escher that he could have heard from no one else at the time, namely the immense horizontal mobility of the rock masses during mountain-building. Was he able to place all this information into a compact model then? Certainly not, as we know from his traditional description of the Alps in his 1862 book. But when he for the first time looked at the tectonics of the entire Alpine chain in 1868, he underlined the dominance of horizontal motions in their behaviour, but in a way very different from what Escher had imagined (Suess, 1868). By that time he must have read the descriptions of the Appalachians by the Rogers brothers (Rogers and Rogers, 1843 or Rogers, 1859 a, b, c, d) and appreciated the general one-sidedness of mountain belts. He also must have read Studer's important 1861 paper and thus had learnt from both sources that fold overturning was an important indicator for tectonic transport. Thus Escher's claim of the existence of two major overturned folds verging towards one another violated the theory of the general asymmetry of mountain belts. Here Suess seems to have followed a dictum later ascribed to Einstein: 'When theory and observation clash, check the observation first!' Suess stuck to that dictum for the rest of his life. Nevertheless, what he was shown as a young men from the top of the Säntis and what he heard from the Swiss master must have created a hurricane in his young mind about the behaviour of mountain chains in general.

It is also interesting that Suess refers to Escher as the more courageous of the two when compared with Studer. This was certainly not the general impression in those days. Escher was regarded as a great observer, but a very diffident man when it came to thinking. Suess realised that this was not so. Escher's diffidence lay not in thinking, but in telling others what he thought. He was deadly afraid of being wrong, probably because of a degree of smugness as Trümpy (1991) recognised. Suess realised that this was no way to behave in science. He took exactly the opposite course and became a genuinely modest scientist, but a prolific writer and encoured his own students to early publication in their careers (cf. Tietze, 1917, p. 345). He realised right in the beginning of his career that most scientific opinions would most likely be wrong, but the only way to correct them would be to submit them to the criticism of as wide a circle of colleagues as possible. In 1890 he said, before an amateur audiance, that 'The natural scientist must know that his work is nothing else but climbing from one error to another, but, with the realisation that getting closer and closer to the truth, similar to one who climbs from crag to crag and, even if he does not reach the summit, he sees the landscape open up before his eyes in ever more majestic sceneries.' (Suess, 1890, p. 4).

Klemun also says that in his autobiography (i.e., in the *Erinnerungen*) Suess nowhere mentions von Hauer, but only Merian and Escher. She believes this reflects Suess' strategy of narrative to put himself in the centre stage. This baseless claim is a result more of the historian's love of intrigue than of the truth. We know from Escher's letter to Weiss, written on 5th November 1854 that von Hauer had simply not been with them on the return trip (see Escher, 1854). In my opinion, doing such a thing as Klemun claims would also have been totally out of character for Suess.

[14] Sir Charles Lyell (1797-1875, Fig. 22) hardly needs an introduction in a geological journal. Suess has often been presented as an antagonist of his views (e.g.,Tietze, 1917; Greene, 1982, p. 191). Greene, inspired by Tietze, went so far as to say that Suess' works 'support generalizations that cut philosophically and substantively against the uniformitarian position and ... pave the way for the demise of Lyellian geology in late-nineteenth-century Europe.' This is entirely untrue, as I tried to show in a number of my previous publications (Şengör, 1982a, b, 2006, 2009b). Suess and Lyell had only one important disagreement: Sir Charles did not allow any global geological events. He attempted to explain the entire geological record by means of local events. He did not exclude from his view of the earth's past catastrophes affecting an entire continent at once, but no more. Suess believed this view mistaken, because, he thought, without global events (such as eustatic transgressions and regressions) the great success of the global biostratigraphy would have been inexplicable, but Suess at the same time emphasised that such global events were not Cuvierian catastrophes. The success of biostratigraphy was a result of the wholesale extinctions of what Suess called 'eco-

FIGURE 23: A black-and white copy of the map to illustrate Sir H. Bartle Frere's notes on the Runn of Cutch and surrounding regions. The map was published in the *Journal of the Royal Geographical Society*, v. 40, between pp. 180-181. For the position of the Allahbund within the Runn, see Fig. 24.

nomic units' of organisms. To judge Suess' position vis-à-vis Lyell one has to remember that in the early fifties of the nineteenth century, when Suess, Ferdinand von Hochstetter and Ferdinand von Richthofen were all volunteers of the Austrian Geological Survey, they considered translating Lyell's *Principles of Geology* into German in order to show, what was to be understood with the word geology (although both the second edition of the first and second volumes and the first edition of the third volume and then again the entire sixth edition of Lyell's *Principles* had already been translated into German by Carl Friedrich Alexander Hartmann {1796-1829} in the years 1832-1834 in three large octavo volumes and 1841-1849 in four small octavo volumes, respectively; on this issue see further: Vaccari, 1998). This shows that as a young man who spoke fluent English, Suess considered Lyell's book as the best guide to teach people what geology was about (Suess, 1916, pp. 113-114).

As I showed in some of my earlier papers and books, as a palaeontologist and stratigrapher, Suess adopted Lyell's actualism as his guiding principle (Şengör, 1982b, 2006, 2009b) and he did not abandon it while undertaking his tectonic studies.

Much has been made of what Suess says on catastrophies in the second chapter of the *Antlitz*. I quote the relevant part below and then discuss what he says:

'Charles Lyell showed, as no one before him, how Nature produces great results by trifling means. But, as Ernst von Baer [1792-1876] has shown in carefully considered words, the standard for small and big as well as for the duration and the intensity of a natural phenomenon is in many cases based on the physical organisation of man. The year is a measure of time furnished by the planetary system; when we speak of a thousand years, we introduce the decimal system and, with it, the structure of our extremities. We often measure mountains in feet and we distinguish long and short periods of time according to the average length of human life and therefore based on the frailty of our bodies; and we unconsciously borrow the standard for the expressions 'intense or 'less intense' from our personal experience.

So our judgement is tied to our physical constitution and we are prone to forget that the planet may be measured by man, but not according to man. The admiration with which one contemplates the building of a coral reef by the little polyp and the hollowing out of the rock by the rain drop, has, I fear, lured into the consideration of the grandest questions of the history of the earth a sort of geological quietism[†††], derived from the pacific commonplaces of everyday life, which does not permit a full appreciation of those phenomena that have been and still are decisive in shaping the present face of the earth.

The convulsions which have affected certain parts of the earth's crust, with a frequency far greater than was till quite recently supposed, show clearly enough how one-sided such a point of view is. The earthquakes of the present day are certainly but feeble reminiscences of those telluric movements to which the structure of almost every mountain range bears witness. Numerous examples of the fabric of large mountain ranges are known suggesting the possibility, and in certain cases even the probability, of the occasional intervention of individual episodes, within the steadyness of the processes, of such indescribable and overpowering force that the imagination refuses to follow the reasoning and to complete the picture the outlines of which the observations of fact furnish.

Such catastrophes have not occurred since the existence of man, at least not since the time of written records. The most powerful natural event for which we have human testimony is known as the Deluge.' (Suess, 1883, pp. 25-26).

At first sight, Suess sounds as if he is talking about real, Cuvierian catastrophes as opposed to a much quieter Lyellian earth. To see whether such an inference is justified we need to look at the text more closely and also in part in the light of Suess' other publications. First let us see what he thought of Cuvierian catastrophes as a young researcher (freely translated by me):

'Although again the influence of the external conditions appear as the main and, most likely, as the only reason of the repeated extinctions of individual species and of entire populations, it is not necessary to fall back to Cuvier's universal catastrophes. Mr. Lyell has the great merit of having shown at length how the processes observable today suffice to bring about considerable changes in the animal and plant kingdoms.' (Suess, 1860b, p. 155).

Suess never believed in the necessity of Cuvierian catastrophes. So how are we to interpret his words in the second chapter of the *Antlitz*? It is important there not to read our own meanings into his words. His critics, who saw him as an anti-Lyellian such as Tietze and Greene interpret the word catastrophe as a disaster of Cuvierian style, a meaning later taken

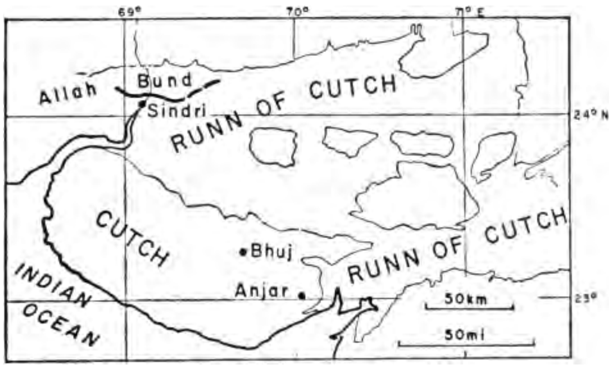

FIGURE 24: The position of the Allahbund that formed during the devastatig earthquake of 16th June 1819 and led to the total submergence of the Fort of Sindree, except for one tower (from Richter, 1958, fig. 31-9).

††† 'Quietism' is a word that had been invented by the Archbishop of Naples, Caraccioli, in a letter he wrote to the Pope Innocent XI on 30th January 1682, to describe the movement started by the Spanish ascetic and Roman Catholic priest Miguel de Molinos (1640-1697). This movement exalted inactivity to allow God to have uninterfered sway over the human spirit. For a short history of this strange doctrine, see Paquier (1910). Suess thought it properly symbolised Sir Charles' view of geology.

over by Leopold von Buch and Élie de Beaumont and confined to a single mountain range or at most to a group of ranges. Leopold von Buch, for example, thought that the Alps had been uplifted with such speed and violence that the marine waters that earlier covered them had been washed northwards in giant floods and cascades carrying with them the exotic blocks. Some of these blocks were allegedly shot from their former locations with such accelerations that they were hurled to the tops of the individual prominences in the Jura Mountains right across the Swiss Plain like so many cannon balls (von Buch, 1827)! When the old Baron explained his idea to Sir Roderick I. Murchison during a joint field excursion, Murchison, himself an anti-Lyellian, found this much catastrophism hard to swallow (Geikie, 1875, p. 75). Élie de Beaumont (1829-1830, 1830a, 1831, 1833) thought that the sudden uplifting of the Andes had led to the simultaneous eruption of all the Andean volcanoes and led to such a global tsunami so as to cause what was later interpreted as the Biblical Deluge. It was this kind of thinking that Sir Charles Lyell had fought against (e.g., Lyell, 1833). Suess no longer experienced the fiercest battles of that war (he was far too young). But by the time he began thinking about them, he had unequivocally placed himself on Lyell's side, as is obvious from his statement that to know what geology means one needed to read Lyell. By that time, however, the concepts had already begun to change their meanings. We know this from Suess' following descriptions of mountain-building. He first takes a recent real catastrophe (during which an entire marine inlet was created and an entire fortress became suddenly submerged except its highest tower) as an example of how individual steps of mountain-building are taken:

'The uplifts in the Runn of Cutch [Fig. 23] and especially the formation of the "Dam of God" *Allahbund* emphasised by Lyell and others [Fig. 24] constitute, after the already mentioned descriptions of Bartle Frère [‡‡‡], something that does not correspond to the way [such uplifts] are explained until now. It seems that here really a wave of the surface was produced [Fig. 25A], and as much as its appearance is different from that of real mountain-building, it must still be mentioned that the profile of the Allahbund [Fig. 25A] really resembles those embyonic folds, which, in my opinion, may be seen as undeveloped mountains and the direction of motion is the same as in the small one-sided mountain the remains of which rise from the Runn of Cutch. The comparison of the profile of the Allahbund with that of the Pays de Braye [sic!] will illustrate what I mean [Fig. 25B]. It is almost as if the mountain-building horizontal motion is in action at this place and the difference between it and the uprising of large mountains lies only in the fact that here the movement is a shallow one, limited to the upper parts of the earth and it is less energetic.' (Suess, 1875, p. 152).

We know today that the terrible earthquake catastrophe of the Runn of Cutch (estimated moment-magnitude, Mw=7.5) of 16th July 1819 was still at least 800 times smaller than the great earthquake and tsunami catastrophe of southeast Asia on 26th December 2004! It still created the 90 km-long, 6 to 9 m-high ridge of the Allahbund which was a result of a shallow northeast-dipping thrust fault related to the Himalayan shortening (see Şengör, 2006). The Fort of Sindree disappeared

‡‡‡ Suess here cites the following references: Journ. Roy. Gegr. Soc. XL, 1870, p. 181-207; see also Wynne, Mem. Geol. Surv. India, IX, 1872, p. 38, forward motion of Allahbund. The relevant passage by Sir H. Bartle Frere is on his pp. 200 and 201:

'Lastly, the size of the sand-ridges in the Thurr [see Fig. 23 herein] seems to me quite inconsistent with any theory of their formation by the agency of the wind. I do not know what may be the greatest measured height of sand hills clearly formed by the wind on any exposed sandy coast like those of Northern Europe, but I believe they never approach to the height of between 400 and 500 feet [120-150 m], which is a not uncommon elevation of the Thurr sand-ridges; and that, be it observed, not in single sand-hillocks, but in continuous ranges of parallel ridges, each of which maintains, for a long distance, nearly uniform elevation. But all the phenomena of the Thurr sand-ridges are consistent with the theory of their formation being due to undulation of a surface like that of the "Runn" or "Put", furrowing the previously smooth surface into billow-like ridges whenever the undulation caused a crack at right angles to the direction in which the earthquake wave was proceeding. We have, indeed, a well-observed and indubitable recent example of the formation of one such ridge by an earthquake undulation in the Allahbund, formed on the Runn by the great earthquake of 1819, and so well described from MacMurdo and Burnes by Sir Charles Lyell. The Allahbund, in fact, is in all respects a perfect, outlying specimen of a Thurr sand-billow of moderate height; and if the process which formed it were repeated, so as to form a sufficient number of similar parallel ridges connecting it with the Thurr, from which it is now a few miles distant, the character of the ridges would be in no respect distinguishable from those of the main portion of the Thurr. Abundant evidence that this is no fanciful theory may be found in the structure of the rocky ridges which wrinkle the plains w. of the Indus. These plains frequently consist of a surface-bed of calcareous sandstone and conglomerate, full of marine shells, and often apparently very little changed, except in elevation from the position it occupied when at the bottom of the ocean. The substratum consists of marls and clays; the surface, sandstone, being of various depth, but often a mere shell only a few feet thick. This crust frequently lies in large plains many miles in extent, and the ridges which traverse it are very generally similar in size and shape to the sand-ridges of the Thurr, or to the Allahbund; while from the presence of the sandstone crust they retain even stronger traces of the mode of their formation. A section at right angles to these ridges generally presents the appearance shown in the annexed diagram [Fig. 25A herein], namely, a long slope of almost undisturbed sandstone, rising to the crest of the ridge, and then a steep scarp down to the level of the plain, where another slope commences rising, while the scarp itself bears the fragments of the broken stony crust, often so little dislocated, that they appear almost capable of being restored to their original position, like the pieces of a jointed map.'

On p. 38 of Wynne I could find no mention of a forward movement of the Allahbund as Suess indicates. On his p. 36 Wynne describes the report of Captain Baker of the Bengal Engineers, in which the asymmetry of the Allahbund was emphasised:

'In 1844 the Allah Bund was visited by Captain (now General) Baker, of the Bengal Engineers. On the 11th of July, he found the 'mound,' cut thorugh by the Pooraun (or Koree), nearly four miles [≈6.5 km] in width, but in other places it was said to vary from two [≈3.2 km] to eight [≈13 km] miles. Its greatest height was on the borders of the lake, above the level of which it rose 201/2 feet [6.2 m]. "From this elevation it gradually slopes to the northward till it becomes undistinguishable from the plain." ...

This is the only observer who mentions that the Allah Bund had any slope on the north side; and the section submitted with his report to the Sind Government shows a slope in that direction from the line of depression northwards, amounting to 19 feet [≈5.75 m] in four miles [≈6.5 km].' (Emphasis by Wynne).

It was possibly this asymmetry of the Allahbund which Suess rightly interpreted as evidence of forward movement, but wrote p. 38 by mistake, instead of p. 36.

(with the exception of a single tower) below the suddenly-formed marine inlet of 2000 square miles south of the Allahbund while, simultaneously, an area of 600 square miles was uplifted north of it (Wadia, 1953, p. 47).

But we know of still larger catastrophes in geology most of which mankind indeed did not experience: think of the evaporation and refilling of immense marine basins such as the Mediterranean during the Messinian (Hsü, 1983a; Ryan, 2009) or the South Atlantic during the Aptian (Burke and Şengör, 1988) or the catastrophic flooding of the North Sea basins during the Permian above the Yellow Sands (Glennie at. al, 2003) or the Black Sea in the Holocene, the only such catastrophe actually witnessed by human beings (Ryan and Pitman, 1998; Ryan et al., 2003; Şengör, 2011), not to speak of the K/Pg Chicxulub impact, which was a Cuvierian catastrophe in the full sense of the word (energy released equivalent to 1.30 x 10^8 MegaTonnes TNT; see Sharpton et al., 1993; Ahrens and Yang, 1995). This last, neither Lyell nor Suess could have conceived of.

Hsü (1983b) showed that there is an inverse energy/frequeny relationship in natural events in geology. The greater the energy involved in a geological event the rarer is its incidence in time. I showed that the same relationship held in the areal extent of an unconformity and its frequency of occurrence in the stratigraphic record (Şengör 1991). From what Suess wrote in the quotation given above, it seems clear that Suess was aware of this relationship in natural phenomena. He probably first became aware of it by 1860, during his study on the ecology of the brachiopods, by a reading of the Swiss botanist Alphonse de Candolle's (1806-1893) *Géographie Botanique Raisonnée* (1855, v. II, p. 1125). He cited it as follows: 'Everything leads to the conclusion that Mr. Alph. de Candolle expressed so clearly: "Races, species, genera, families have an unlimited duration, i.e. they have no intrinsic reason to end at a given time. They last until the external causes influence them slowly or quickly. This happens frequently for the races, less frequently, even rarely, for the species, very rarely for the genera and especially for the families"' (Suess, 1860b, p. 154-155). Alphonse de Candolle's interpretation was based on his father Augustin de Candolle's (1778-1841) distinction in his *Géographie Botanique*, between the normal dwelling place ('*station*') and the maximum geographical distribution ('*habitation*') of the species (de Candolle, 1820, p. 383; see also Bowler, 1992, p. 274).

Let us now take a look at what he said about the temporal aspect of the mountain-building in the Alps:

'Such examples ... from the Alps ... prove that back to a certain point in time, which reaches deep into the Mesozoic Epoch the region of the Alps has been the site of large dislocations. This is also the place to remind us how much richer the Southern Alps are in eruptive rocks of all ages than the Northern Alps and this situation implies that the processes in earlier times were essentially the same as those in later epochs.

The question is whether the causes through which the mountain chains originated are still active today and this question, ... up to a certain degree, I have to answer in the affirmative.' (Suess, 1875, p. 56).

Suess wrote later in the same book that 'the duration of the force of mountain-building goes through many epochs in individual chains' (*ibid.*, p. 146). He wrote also that 'it is true that individual acts of uplift of our mountains were sudden' (*ibid.*, p. 154-155); but this was only in conjunction with earthquakes

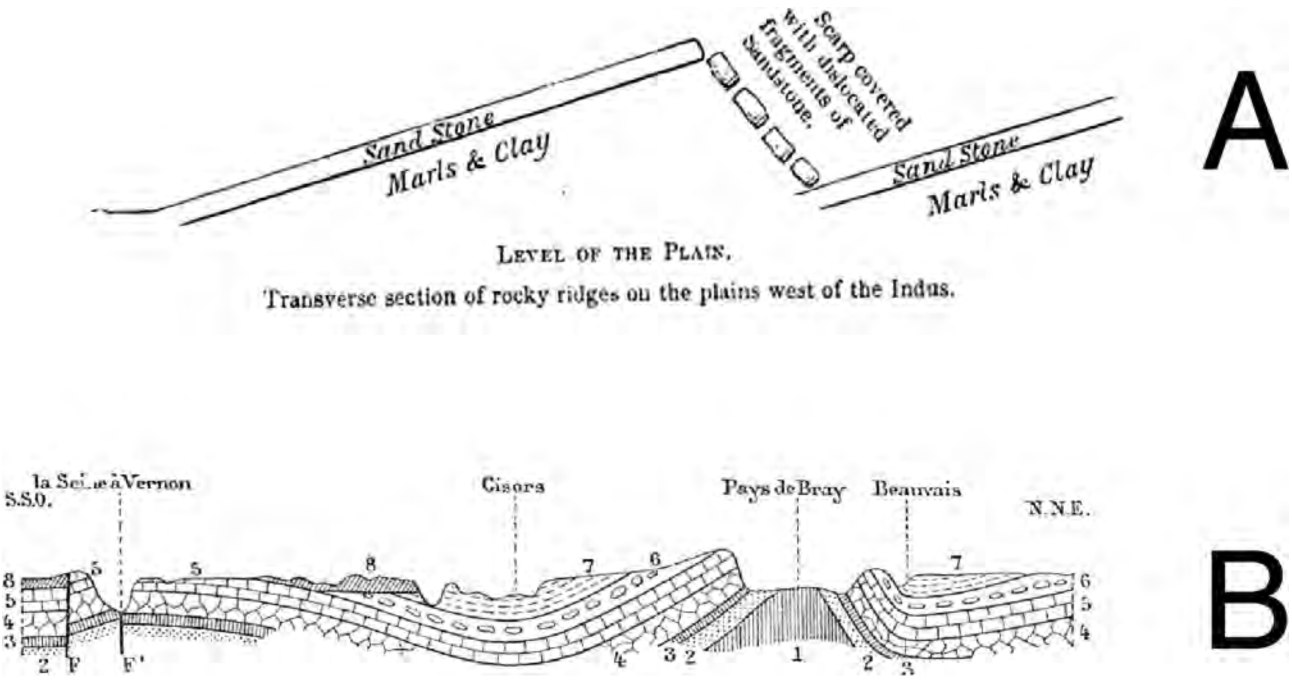

Figure 25: A: Sir H. Bartle Frere's view of the structure of the Allahbund (south to the right). B: Edmont Hébert's cross-section across the asymmetric anticline of Pays de Bray (from Vélain, 1892, fig. 377). Suess compared the sketch of Sir H. Bartle Frere with a similar cross-section of the Pay de Bray anticline.

similar to that which raised the Allahbund. In the immediately following sentence he continued: 'Many earthquakes can perhaps be viewed as the local release of stresses that are perennially present as a consequence of the contraction and during such an impact of a wave the mobile upper surface of the earth could be accelerated forward. Its course and uparching may be compared with that of a mountain.' (*ibid.*, p. 155).

Only after having looked at the development of his ideas on the behaviour of the planet can we finally see what Suess means when he says catastrophe. For him a catastrophe is an event that changes the landscape and may lead to local upset in the fauna and the flora of a region. But he thinks such catastrophes are commonplace. Some are very rare. Some are so rare that mankind has not witnessed their kind. This does not mean he was a catastrophist in Cuvier's sense or that he had given up Lyellian actualism. Had Sir Charles himself not admitted that a sudden discharge of the waters of the Great Lakes in North America may catastrophically inundate much of the continent and that such an event would be perfectly acceptable within his theory that the past causes were no different from the present-day (he wrote 'actual') causes (Lyell, 1830, p. 89)? In his first Presidential Address to the Geological Society in London, Sir Charles stated opinions showing that Suess' way of viewing the earth was very much in the spirit of Lyell's thoughts:

'During the same tertiary [sic!] periods there have been vertical subsidences as well as elevations of the same areas; and we have every reason to believe that the large part of the globe (comprising nearly three fourths of its superficies), which is covered by water, has undergone, in equal periods of time, oscillations of level not inferior in degree to those to which the continental spaces have been subjected. If therefore we were to confine our thoughts to the more outward modifications in the shape of the land or bed of the sea, and all the changes of climate and fluctuations in organic life inseparably connected with movements which have amounted, in some cases, to more than two miles vertically in one direction, besides the lateral displacements of rocks and their denudation by water, the series of events would seem endless, and their magnitude not easily to be exaggerated. But it is evident that these superficial mutations are trifling in amount in comparison with revolutions which must have been going on simultaneously in the inferior parts of the earth's crust. The reality of these changes is certain, although their nature may be obscure.' (Lyell, 1850, SS. xxxix-xl).

There is thus no reason whatever to think that Suess was not a Lyellian in his tectonic thinking. In fact everything we have from Lyell and Suess shows precisely the opposite. However, Tietze (1917) claimed that two of Suess' own students, Theodor Fuchs (1842-1925) and Viktor Uhlig (1857-1911) considered Suess an anti-Lyellian. It is easy to dismiss Tietze, because he was silly and incompetent[§§§], but not Fuchs or Uhlig, two of the Viennese giants. It is therefore imperative to see what they really said. Neither said what Tietze claims they said in their rave reviews of Suess' *Antlitz*.

Let us start with Fuchs' review, who knew Suess for much longer than Uhlig did. He contributed it to the Viennese daily newspaper *Neue Freie Presse* in two instalments just after the completion of the fourth (actually labelled as III/2) volume of the *Antlitz* in 1909. The first instalment was published on 4th November 1909 (Fuchs, 1909b) and the second instalment the following week, on the 11th November (Fuchs, 1909a). The first instalment is subtitled 'The beginning and the preparation for the work.' Here Fuchs begins really at the beginning by portraying the sad state in which the teaching of natural sciences had found itself in the high-schools of the Austro-Hungarian monarchy during the first half of the nineteenth century. He says that the teaching of the natural sciences looked very unsatisfactory compared with that of the classics. He argues that this was because, at the end of a high school education, the students were able to read Greek and Latin, but had acquired no usable skill in the natural sciences, because those were taught as disjunct bits and pieces of information. He says Suess rebelled against this situation already when he was an employee of the Imperial Mineral Cabinet

Figure 26: Léonce Élie de Beaumont.

[§§§] Tietze was also given to personal attacks on people he disapproved of. His disapproval of others seem not uncommonly motivated by jealousy. See Rudolf Hoernes' criticism of his personal attacks on Mojsisovics and Bittner (Hoernes, 1882).

and thought that geology would be the best way to teach the natural sciences in a synthetic whole, because geology, by its nature, embraces them all. But there were no suitable textbooks. So Suess resolved to write one. He had originally considered with his friends Ferdinand von Hochstetter and Ferdinand von Richthofen to translate Lyell's *Principles of Geology* into German. This project fell through, because von Hochstetter left with the *Novara* Expedition to go around the globe and von Richthofen also went abroad with the Eulenburg Expedition to south and east Asia. Suess soon realised however, that a translation of Lyell's book would not be suitable for Austrian schools, becuase Sir Charles had hardly dealt with examples from the Austrian Empire. So, Suess decided to adapt the book by replacing its extra-Austrian examples with Austrian ones. He estimated that a labour of three years would suffice to produce such a book, as the Austro-Hungarian Geological Survey and the Imperial Mineral Cabinet would surely be happy to supply the necessary material. Any extra material, Suess could procure himself by appropriate excursions.

Fuchs said that things did not turn out as Suess had hoped, however. Wherever he went he saw things not at all mentioned in Lyell's book or not in accord with what he wrote. The first problem was that the stratigraphy known from England, France and even Germany could not be applied to the Alps. Bohemia was much less of a problem, as its Palaeozoic stratigraphy could be correlated with that in western Europe and even eastern North America. In the Mesozoic, the Alps showed facies totaly different from that in those places. This great difference between Bohemia and the Alps, Fuchs says, had made a very profound impression on Suess and actually spurred him to his comparative geological studuies. The flysch north of the Alps looked unlike anything known farther north. The Carpathian sandstone area did look like the Palaeozoic sandstone areas of Belgium, but in these latter places the sandstones had coal in them, of which there was no trace in the Carpathians. Wherever Suess looked, even in the Tertiary deposits, Lyell's book looked to be of no help and Suess had to accept the fact that he had to begin from the beginning. This did not deter him, however. He began criss-crossing his country and publishing his results in small papers. One of the earliest results was that the continental uplift hypothesis of Lyell was untenable; instead, it had been the sea-level that had moved up and down in the past. Suess soon showed that the mountain uplift hypothesis by axial magma injection (although not instantly as Leopold von Buch and Élie de Beaumont would have it, but imperceptibly slowly), accepted by Lyell, did not work. Neither was the idea that mountain ranges were bilaterally symmetric about an intrusive axis correct. All these ideas were either new or they were not in Lyell's book. But for all these novelties, Suess never deviated from Lyell'is idea that great results can be obtained by the summation of small events. In other words, he never abandoned actualism, but he did abandon Lyell's conviction that there were no global geological processes.

In the second part of his review (Fuchs, 1909a), Fuchs presents the book *Das Antlitz der Erde*. Here he asks the question: did Suess follow Lyell's uniformitarianism (not actualism) to the bitter end? He says, no, because Suess thought he could see fluctuations in the intensity of volcanic activity since the Cambrian: he thought both the Palaeozoic and the Cainozoic had more intense vulcanicity than the Mesozoic. He also realised that large trans- and regressions were global. He thought orogeny was globally more widespread at the end of the Carboniferous and at the end of the Miocene. None of these could have been brought into agreement with an absolute uniformitarianism, or, better expressed, with a steady-state earth at all times. This however, never meant that one should abandon Lyell's methodology and revert to catastrophism. Suess practiced geology as Lyell did and showed how the record could be interpreted in terms of global events. Suess never admitted global catastrophes. At the end of his review, Fuchs points out Suess' dissatisfaction with Darwin's theory of natural selection, because it neglected the geological factors through time. However, Fuchs points out, Suess did not discuss the issue in the last chapter of the *Antlitz*, devoted to the life on earth, because he did not want to get into the rapidly expanding field of evolutionary biology of his time. Fuchs says many of the readers of the *Antlitz* had been eagerly awaiting Suess' verdict on Darwin's theory and they were disappointed. The master did not touch it in the *Antlitz*. Only in his farewell address, Suess greatly praised Darwin's theory, but said that the geological record did not show the sort of haphazard evolution one would expect from his theory. Times of transgression were times of rapid evolution and increasing biodiversity, whereas times of regression were times of global extinctions and decline in biodiversity (Suess, 1902). So far Fuchs' review.

Let us now read Uhlig. This giant mentions Lyell's 'actualism' as he calls it, only in the introduction of his wonderful review:

'The *Antlitz* der Erde has a long history behind it. One cannot fully appreciate it if one does not follow it on its way. When the first volume appeared in 1882 [*sic*!], despite all the progress in stratigraphical, palaeontological and petrographical fields, geology was not in a very satisfactory situation. Lyell's actualism, which recognised no other standard than the experience of today, threatened to deteriorate into a geological quietism. Lyell's teaching had somewhat diverted the geologists from the great tasks and made them gradually incapable of being surprised, of seeing puzzles and of evaluating them. Through a brilliant study of the Deluge, put like a motto in the opening of the first volume, Suess showed the trifling character of this catastrophe, which was the greatest in the memory of man, and pointed out its immeasurable distance from the geological events of the past. Like a warning to the geologists this cautionary note permeates the entire work not to look at the grandeur of Nature with too small points of view.' (Uhlig, 1909a, pp. 103-104.)

What Uhlig says Suess criticised is not Lyell's teaching, but those who abused it to bury their heads in the local, mundane processes of the present time. Suess recommended to enlarge the view and to look at the larger processes, at the entire planet. Nowhere did he criticise Lyell's methodology, which he himself loyally followed. The various kinds of uniformitarianism, as outlined by Gould (1965), Hooykaas (1963, pp. 3-4, 1970, pp. 7-9) and, following them, Rudwick (1971) have thoroughly clouded and befuddled our understanding of Lyell. Their distinctions have limited relevance to Hutton's or Lyell's thinking, for both were interested in combatting hypothetical reconstructions of global or near global and very rapid events of truly catastrophic dimensions ("great changes, of a kind and intensity quite different from the common course of events, and which may therefore be properly called catastrophes:" Whewell, 1837, p. 606, 1857, p. 506, italics Whewell's), the occurrence of which in today's world would be unthinkable. All Hutton and Lyell were trying to do was to show that the geological processes active today and what we knew of the geological record then did not necessarily require the incidence of instant world-wide catastrophes in the past. Suess was in complete agreement with this view, as the quotations I give above show. None of them could have possibly thought of

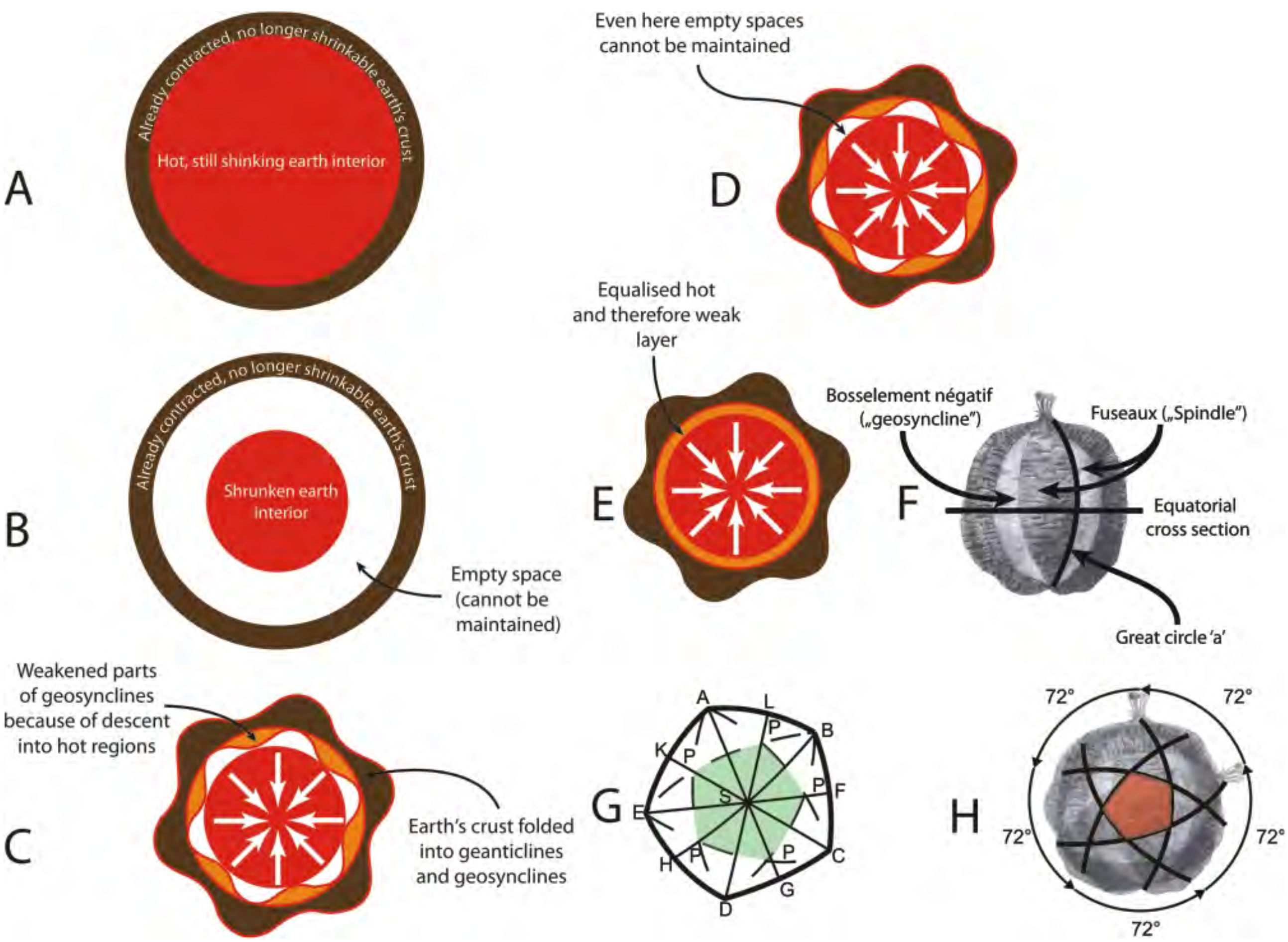

FIGURE 27: Sketch drawings by Şengör of Élie de Beaumont's theory of terrestrial thermal contraction. A: State in which no contraction has yet occurred. B: A theoretical stage where contraction has occurred and created an empty space between the already contracted, no longer shrinkable crust and a shrunken, still hot interior. Because maintaining such an empty space within the earth is impossible, the crust shortens by creating large folds which Élie de Beaumont called negative and positive *bosselements* corresponding to the later geosynclines and geanticlines of Dana (1873), respectively. C: The bottoms of the negative *bosselements* become hotter as they descend farther into the hot interior and consequently they become weakened. Suess pointed out in the *Antlitz* that nowhere on earth such structures could be documented, now or in the past. D: The spaces created during the C phase also cannot be maintained. E: Because the spaces created during the C phase cannot be maintained, the weakened lower parts of the negative bosselements flow and fill the voids. This leads to effective crustal thinning under the negative bosselements. F: An experiment illustrating an experimental production of *fuseaux* using a shrinking baloon, on which glued contractable and non-contractable materials were made to alternate along meridional lines (from Daubrée, 1879, fig. 128). These semicircular 'slices' allegedly correspond with those of a maximum width of some 2000 km. on the face of the earth, along which, according to Élie de Beaumont, narrower mountain chains commonly alternate with basins. Élie de Beaumont thought that the *fuseaux* had to form as a consequence of the shrinking of the planet along great circles and because displacement could not be transferred from one great circle to another (because at the time strike-slip faults were neither hypothesised nor yet recognised—thus, Élie de Beaumont's *fuseaux* had to form because he lacked conceptually what is in essence transform faults in a contractionist framework! Tuzo Wilson had such faults in his 1954 paper: see his figs. 8 and 16). G: Different episodes of contraction employed differently orientated fuseaux on the face of the earth, the collective traces of which formed his famous 'pentagonal network' (réseau pentagonal: see Élie de Beaumont, 1852 passim). H: The placement of the pentagon shown in Fig. G on the globe, which was created by different episodes of contraction creating differently orientated *fuseaux*. Succesive episodes of contraction creating *fuseaux* orientated 90° from one another cannot create a pentagonal network. For that the *fuseaux* need to keep a separation of 72° from one another. I cannot understand why Élie de Beaumont thought that the mountain trends created by succesive episodes of contraction had to be 90° apart (See Saint-Claire Deville, 1878, p. 520).

something like the Chicxulub impact, which essentially vindicated in part Cuvier's interpretation of mass extinctions.

[15] Suess here voices the same criticism of Lyell's *Principles* as his two great students, Fuchs and Uhlig, emphasised in their reviews of the *Antlitz*. Lyell had not touched the important question of mountain-building. With it he neglected an entire class of phenomena in geology, namely those of tectonics (Ellenberger, 1994, p. 311, expressed this most picturesquely: 'Lyell's allergy to tectonics'). These were, according to Suess, the greater tasks of geology. In fact when he was asked to speak before the Society of Natural Science in Görlitz, he entitled his talk as *Ueber neuere Ziele der Geologie*, i. e., on the new objectives of geology, and in it he talked about the tectonic subdivition of Europe as an example of the tectonic subdivision of continents with a view to understanding the origin of continents using comparative geological studies as a basis (Suess, 1893a). It was this grand task that Lyell had neglected. Escher, the avid field-worker, made the necessary observations to complete that task with skill, yet failed to communicate them to the world of science in the usual and then the most efficient way: through publication and he shied away from theory. Suess, mentioning these two giants of geology and emphasising that ' in the contrast of their qualities the whole wide field of activity in our glorious science is brought into view' also implicitly criticises the times preceding his own. It was a time when looking at the planet as a whole was shunned. It is this he finds necessary to correct, neither Lyell's nor Escher's methodologies. He recommends both be pursued, but at the scale of the entire planet and its history.

[16] Élie de Beaumont (1798-1874; Fig. 26) was one of the towering figures not only of French geology, but of geology in the whole world during the 19th century. He revived the contraction theory, invented the idea of geosynclines (but not the term), introduced Steno's method of dating mountain-building events into modern geology (see his important paper on Steno and some other ancient authors: Élie de Beaumont, 1832, especially footnote on p. 332 and his plate 12, in which he reproduces Steno's figure showing the successive stages in the structural development of Toscana and of the entire earth in Steno's interpretation, which was a precursor of his own ideas on dating mountain-building as he says in his just-mentioned footnote) and was the co-author of the first geological map of France and its magnificent explanatory volumes, which Suess frequently consulted. Élie de Beaumont remained a staunch defender of the catastrophist views of Cuvier and Leopold von Buch to the bitter end and not infrequently came into conflict with Lyell and the uniformitarians in his country such as Constant Prévost (1787-1856). In almost all theoretical positions he adopted, time proved him wrong, and, in his later years, he became more of an hindrance than a help for geology, but, as Suess says, no one can deny the immense beneficial influence he also had on geology.

In his letter to Sollas, Suess does not mention that he also knew Élie de Beaumont personally. The reason can be guessed at in his relation of his meeting with him in Paris in 1856. During his second visit, Suess arrived in Paris on the 18th May and left for Caën on the 24th. He returned to Paris on the 3rd and left on the 5th June. He does not say when he met Élie de Beaumont, but the description of his activity in Paris in his *Erinnerungen* suggests that it was probably in May. He spent most of his time with the great palaeontologist Gérard Paul Deshayes (1795-1875) during his visit in June. He describes his meeting with Élie de Beaumont with the following words: 'I introduced myself to the great geologist Mr. Élie de Beaumont, who received me with a condescending attitude and the allure of a superior being. Despite this childish behaviour I have never ceased to respect him because of his deep knowledge and his constant attempt at a global view' (Suess, 1916, p. 127). Suess combined Lyell's methodology with Élie de Beaumont's global view. He liked Lyell personally and obviously did not like Élie de Beaumont.

[17] Suess' reference to Élie de Beaumont's book is not entirely correct. What did appear in the *Dictionnaire* was a long but truncated article simply entitled 'Systèmes de Montagnes' (Élie de Beaumont, 1849). The article actually appeared in the 137th instalment (=*livraison*) of the *Dictionnaire* on 1st September 1849 (Élie de Beaumont, 1852, p. V; see Evenhuis, 1990, for the problems associated with the dating of the individual livraisons of the *Dictionnaire*; Evenhuis, 1990, was in fact unable to find the date of publication of the 137th *livraison*), not in 1852. This instalment is in the 12th volume. The cover of this volume bears the date 1849, whereas the title page 1848!

Charles d'Orbigny (1806-1876), the younger brother of the great Alcide d'Orbigny (1802-1857) and the editor of the *Dictionnaire universel d'histoire naturelle*, invited Élie de Beaumont in 1841 to contribute a number of articles on geology. At the time, Élie de Beaumont was engaged with his friend Dufrénoy in the publication of the geological map of France, so could only promise 'a cooperation on the long run and with little activity' (Élie de Beaumont, 1852, p. II). However, he did commit himself to writing the article on *montagnes*. Yet when the time came in 1846 for him to deliver the article on *montagnes*, Élie de Beaumont found himself in the middle of writing the second volume of the explanatory notice of the geological map of France and totally unable to do anything else. D'Orbigny, deferred the article to a later letter by putting in the place where the original article was expected *Montagnes—Voyez Soulèvements et Révoltions du globe* (Mountains—See uplifts and revolutions of the globe: v. 8, 1847[1846 on the title page], p. 340). As Élie de Beaumont's other commitments dragged on, poor d'Orbigny found himself in the necessity of putting *Revolutions du Globe—Voyez Systèmes de Montagnes* (v. 11, 1849[1848 on the title page], p. 84) and *Soulevements—Voyez Systèmes de Montagnes et Terrains* (v. 11, 1849, 1848 on the title page], p. 696) as their time came in turn. In 1848, the second volume of the explanatory notice of the geological map of France already at the publisher, Élie de Beaumont thought he could finally get on with fulfilling his promise to d'Orbigny. He had gathered much material on the subject, some of which he had published in the interim (see especially Élie de Beaumont, 1847, 1850). Alas, other occupations still prevented Élie de Beaumont from writing his article. Further deferments in the *Dictionnaire* not being possible, d'Orbigny asked the printer to leave enough space for Élie de Beaumont to fit his article into and continue printing the rest of the issue. When Élie de Beaumont finally finished his article, it was found to be too long for the space he originally had agreed to. Thus what appeared in the *Dictionnaire* finally on 1st September 1849 was a version truncated at the 'Système de l'Erymanthe et du Sancerrois' (i.e., at page 311 of the 12th volume of the *Dictionnaire*, which corresponds with p. 528 of the *Notice*)! But p. 528 brings the *Notice* only to the end of its volume I. But the most interesting parts of the *Notice* are in the 3rd volume, where Élie de Beaumont gets into the theoretical questions of global tectonics, which the readers of d'Orbigny's *Dictionnaire* never got to see. It is clear that there was no question of Élie de Beaumont's material, which eventually did not make it into the *Dictionnaire*, not being 'necessary or appropriate' for the *Dictionnaire* as Greene (1982, p. 116) thought. Élie de Beaumont had simply become a victim of deadlines!

[18] Suess' point that Élie de Beaumont's 'geometrical theories' found little appreciation outside France' touches upon an extremely important subject in the development of his own thinking on tectonics. In note 8 to his 'first guide' above, I cite his statement published in *Die Entstehung der Alpen*: 'After we have given up a geometric system and accepted the one-sidedness of the movement...'. The geometric system he says he had given up was Élie de Beaumont's *réseau pentagonal*, i.e., the pentagonal network (Élie de Beaumont, 1850). Actually, I do not think Suess ever seriously entertained the idea that the *réseau pentagonal* could possibly be true. We have no evidence whatever in his writings that might make us think he took that eccentric theory seriously. But in consideration of ist author, he had to discuss it and formally refute it, which he did in the *Entstehung*. However, Suess' discussion and refutation were so brief and because almost nobody else has ever given a detailed description of Élie de Beaumont's theory, it is very little-known. His students Alexandre-Émile Béguyer de Chancourtois (1820-1886) and Charles Joseph Saint-Claire Deville (1814-1876) attempted to describe it, but failed to do so adequately. Béguyer de Chancourtois (1875) gave a brief sketch with only one figure. Saint-Claire Deville, in his lectures in the Collège de France, also tried to describe it but, because his book based on these lectures contains no figure at all explaining the geometric constructions he describes in his last two chapters****, its message is difficult to understand by those readers who do not know anything about the *réseau pentagonal* to begin with (see Saint-Claire Deville, 1878). I present below a detailed description, mostly following de Lapparent's (1839-1908; he was a student both of Élie de Beaumont and Béguyer de Chancourtois) thorough and illustrated presentation in the first edition of his classic *Traité de Geologie* (1883, pp. 1239-1244), with a view to showing what Suess criticised and refuted. In his search for a *vera causa* for the global cataclysms and the supposed simultaneous mountain-building catastrophes, Élie de Beaumont started with the then popular assumption that all the planets around the Sun had to have begun their lives as incandescent bodies and were now in different stages of refrigeration (see Élie de Beaumont, 1829, footnote on pp. 15-19). So far Suess was in agreement, at least as early as 1875.

Élie de Beaumont then thought that the outermost part of a planet would stop cooling when the supply of heat from the interior equals its heat loss to space. At that point the outermost shell would stop contracting (Fig. 27A). But the interior, being hotter, would continue to cool and thus continue to shrink. This would tend to create an empty space between the outer, already contracted, no longer shrinkable shell and the shrunken interior as shown in Figure 27B. Because an empty space within the earth cannot be maintained, the outer shell will conform to the new size by diminishing its diameter which will be expressed as tangential shortening. Élie de Beaumont thought

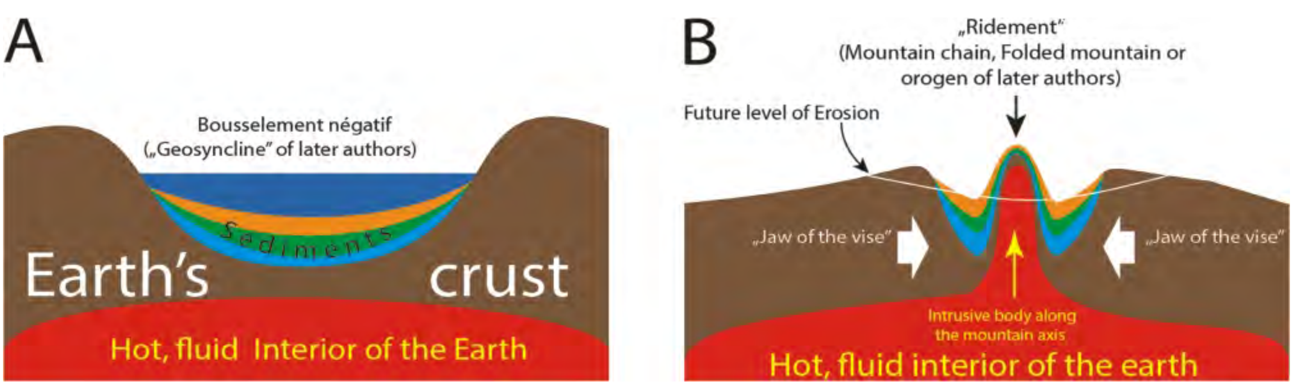

FIGURE 28: A: A negative bosselement between two positive bosselements as a preparatory stage for mountain-building according to Élie de Beaumont (1828, 1852). This is the essence of the gosyncline idea. B: Mountain-building according to Élie de Beaumont (1829, 1829-30, 1852), The two positive bosselements crush the intervening negative bosselement. In the process a granite is squirted out like a *boutonnière* along the axis of the mountain range (ridement).

**** Unfortunately, Saint-Claire Deville died before he could finish reading the proofs of his book. He had advanced only to the third chapter before he expired, whereas the *réseau pentagonal* is the subject of chapters eighteen and nineteen. The book was seen through publication by F. Fouqué, who wrote the preface explaining the circumstances in which the book was published.

that this shortening would take the form of large-scale folds, which he called *bosselements* (i.e., bulgings); anticlinal folds were called positive *bosselements* and the synclines negative *bosselements*. As the bottoms of the negative bosselements would sink into hotter regions, they would be heated and weakened (Fig. 27C). But even in such a configuration, empty spaces would still remain, which cannot be maintained (Fig. 27D). The weak part would therefore flow to redistribute itself under the folded outer shell, effectively thinning it under the negative *bosselements* and thus further contributing to their weakening (Fig. 27E).

Fig. 28 shows the manner in which mountain belts form in Élie de Beaumont's version of the contraction hypothesis: Fig. 28A shows the preparatory stage when a negative *bosselement* between two positive ones build a trough which would have a weak floor for the reasons explained above. Fig. 28B shows what happens when the contraction-derived stresses finally exceed the strength of the floor of the trough leading to ist failure and crushing between the two positive *bosselements*, which Élie de Beaumont likened to the jaws of a vise: the contents would be folded and granitic intrusion would build the axis-by piercing the central anticline. This piercing granite, Élie de Beaumont compared with a *boutonnière* (buttonhole in English).

Since strike-slip faults were unknown, Élie de Beaumont thought that the bosselements would take the form of spherical gores, which he called *fuseaux* (spindles; Fig. 27F). Every episode of contraction would form its own set of *fuseaux* and different episodes of contraction would generate *fuseaux* differently orientated on the surface of the sphere (Fig. 27G and H).

Now the graduate of the highly mathematical École Polytechnique and a student of the distinguished mineralogist André-Jean-Francois Marie Brochant de Villiers (1772-1840) thought that if mountain belts would form on a contracting globe following certain definite directions (i.e., the *fuseaux*), they ought to follow the rules of a symmetry that would be most suitable for the surface of a globe. This conviction was perhaps subconsciously inspired by but expressly not based on the crystallography of the Abbot René-Just Haüy (1743-1822) who had shown that every mineral species has a specific crystal form of its own that can be discovered by a continuous peeling off of layers of molecules, the thickness of which is always characterised by simple rational numbers (Haüy, 1784). Élie de Beaumont thought that crystals all basically had a quadrilateral symmetry and could not attain a pentagonal symmetry, but that certain regular arrangements of molecules could do so (Élie de Beaumont , 1852, v. III, p. 1222). Élie de Beaumont thus believed that every contracting solid would tend to take on a pentagonal form (he totally ignored gravity and the fact that solids can also flow; he could have known both had he remembered Newton or at least his countryman Laplace, who had such a great influence in his *alma mater*, the *École Polytechnique*††††). Because of that belief, he tried to find a solid form that most closely resembles a sphere. The problem for him therefore reduced itself to finding a symmetry rule that would allow the division of the surface of the globe into smaller equal-area surfaces possessing the richest symmetry. A spherical regular pentagon delimited by five great circles has the highest number of symmetry elements about a point on the surface of the globe as shown on Fig. 27G. This fundamental regular pentagon would form on the surface of the contracting sphere by five successive episodes of shortening as shown in Fig. 27H.

Having 'demonstrated' how mountain ranges needed to be rectilinear in their trends, but also to delimit regular pentagonal areas on the surface of the globe, Élie de Beaumont proceeded to investigate, entirely on the basis of his geometric model, how the many directions of rectilinear mountain ranges may be built:

When one connects each pair of the alternate corners of the regular pentagon ABCDE (Fig. 27G) with new great circles forming the diagonals of such spherical rhombi as ABCS or BCDS etc., one realises that these great circles define a smaller pentagon PPPPP within the fundamental pentagon (green pentagon in Fig. 27G). The surface of this smaller pentagon can be made to occupy one-twelfth of the surface of the globe, so Élie de Beaumont 'found' that a contracting globe would tend to assume the shape of a pentagonal dodecahedron (Fig. 29). For a well-educated scientist of the first half of the nineteenth century, this 'discovery' was a most satisfying result, because not only is the pentagonal dodecahedron a very common mineralogical crystal form, but also one of the five ideal Platonic solids (discussed in Plato's *Timaeus*, 53D-54C) that are the only regular, convex polyhedra with congruent faces of regular polygons having the same number of

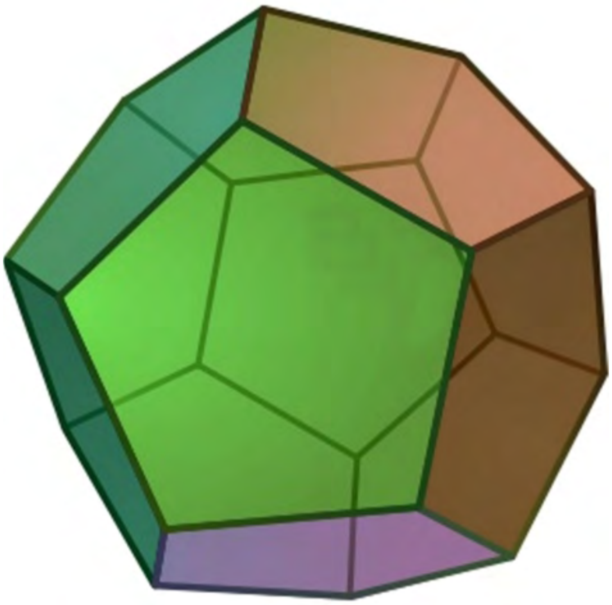

Figure 29: Pentagonal dodecahedron (copied from http://en.wikipedia.org/wiki/File:POV-Ray-Dodecahedron.svg).

†††† Laplace's insistence on a mathematical approach to science and his immense success in it, obviously influenced Élie de Beaumont; but he seems to have overlooked Laplace's second emphasis: a thorough grounding in physics and chemistry.

faces meeting at each vertex as already shown in antiquity by the Athenian mathematician Theaetetus (417-369 BCE) (Bulmer-Thomas, 1981) and depicted in the 13th Book of Euclid's Στοιχεῖα (*Elements*), propositions 13 to 17; the dodecahedron itself is discussed in proposition 17 (Euclid, c. 300 BCE [2007]). Moreover, Plato remarked in *Timaeus* that the demiurge had arranged the heavens using the pentagonal dodecahedron as as a geometrical basis.

But there was more to Élie de Beaumont's theory of mountain-building: he also believed to have 'discovered' a hierarchy of mountain ranges! This 'discovery' resulted from the following geometric considerations:

As every regular pentagon has five edges of equal length and each one of these edges also belongs to a neighbouring pentagon, the total number of edges in a pentagonal dodecahedron (Schläfli symbol {5,3}) is 30 extending between 20 vertices. These faces have dihedral angles of 116.57°. Every pair of this 30 falls, when projected onto the surface of a sphere, on a great circle. Élie de Beaumont viewed these 15 great circles as the primitive symmetry elements of his pentagonal system. He then constructed a regular pentagon on a flat surface tangential to the globe as shown in Fig. 30. On such a gnomonic projection, great circle arcs appear as straight lines. He then connected the midpoints of two adjoining edges of the regular pentagon, as, for example, L and F in Fig. 30. He also connected a corner with two other corners that are not its neighbours, as, for instance, A with C and D (Fig. 30). The great circles thus produced are subordinated to the general symmetry of the system. These new great circles intersect the fundamental great circles at points M, N and P (Fig. 30). When one connects these with the centre of the pentagon, there appear further great circles subordinated to the general system. Such a geometric play can be continued as long as one wishes. Within this system, Élie de Beaumont established his mountain range hierarchy, i.e., the relative importance of the great circles along which mountains had formed, as follows:

He took four neighbouring spherical regular pentagons with its centres at S, S', S" and S"' as shown in Fig. 31. When the edge AE of the first is lengthened in the direction of S', it forms the apothem EM of the second. Similarly, if we lengthen B'C, we form the apothem CL with respect to the edge AE. When the C'M is lengthened, it forms a perpendicular at N and thereby bisecting BC. Through this procedure we obtain a spherical right triangle formed by the arcs LM, LN and MN (Fig. 31). Therefore, these three circles represent planes, which, when taken through the centre of the globe, constitute three adjacent faces of a cube. This means that the fifteen principal great circles represent five cubes disposed symmetrically with respect to one another. Let us recall that these fifteen great circles are the primitive circles of Élie de Beaumont. They are grouped into three rectangles taken three at a time and every one of them has a pole on the surface of the globe as M or N, i. e., the midpoints of the edges of the pentagon. It is also possible to construct similar geometric groupings of the primitive great circles and it is seen that some form octahedrons

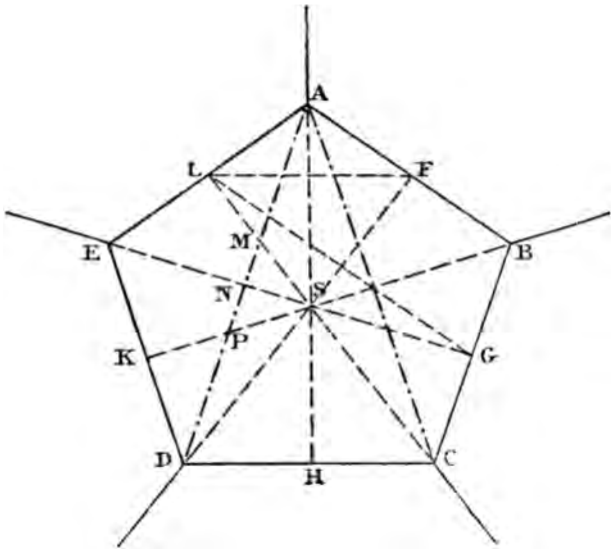

FIGURE 30: The basic pentagon in Élie de Beaumont's *réseau pentagonal* (gnomonic projection, from de Lapparent, 1883, Fig. 604, p. 1241).

while others form rhombohedric dodecahedrons. Hexatetrahedronic groupings correspond to the 24 surfaces of a hexatetrahedron which is subordinated to a cube.

In addition to the surfaces and solids mentioned above, Élie de Beaumont also established 362 main points in his system: 12 centres of pentagons, 20 vertices of pentagonal dodecahedra, 30 mutual intersections of 15 primitive great circles, 60 points at which two octahedra touch a primitive circle, 60 orthogonal intersections between primitives and octahedra, 60 orthogonal intersection points between octahedra and dodecahedra and 120 orthogonal intersection points between octahedra and rhombic dodecahedra. Figure 32 shows Élie de Beaumont's complete pentagonal network.

So far we have reviewed only the geometry of the pentagonal network (*réseau pentagonal*) . Once this network was worked out geometrically, it needed to be placed on the terrestrial globe in such a way that, ideally, all its elements would find corresponding structures in the tectonics of the earth (Fig. 33A). As we saw above, Élie de Beaumont assumed that the dislocations of a given time interval would be concentrated within a single *fusée*, in which they would appear as a series of parallel wrinkles (he called them *ridements*; see Fig. 28B). A great circle drawn parallel to these 'wrinkles' and bisecting the *fusée*, is called a 'great circle of comparison' (*grand cercle de comparaison*) of a mountain system. When a few such great circles of comparison were established, one tried to see whether they coincided with some of the great circles of the pentagonal network and if they did, whether such coincidences led to others.

Élie de Beaumont thought that two succesive episodes of contraction would form structures at high angles to one another and therefore one used such orthogonality relationships (which Élie de Beaumont called *traits de carrés*, i.e., '*quadratic traits*,' of which there are said to be 270) while installing the theoretical pentagonal net onto the terrestrial globe.

Élie de Beaumont called the great circle connecting the island of Teneriffe in the Canary Islands (which houses the active volcano of Mount Teide, the third highest volcanic mountain in the world when measured from its base on the ocean floor) with the Etna in Sicily and which also goes through the volcanic Aegean Islands (Methana, Milos, Santorini, Nisyros), 'the volcanic axis of the Mediterranean'. This was one important principal great circle. Élie de Beaumont considered it important, because it was also parallel with the main trend of the Alps between Geneva and Vienna (see Fig. 33B and 34). Another important great circle was the one connecting the Etna with the Vesuvius and the Mauna Loa in the Island of Hawai'i, the largest volcano on earth. This great circle makes an angle with the first one which deviates from a right angle by only 8'. Élie de Beaumont used these two great circles as a quadratic trait which corresponded to one in his geometric pentagonal net, formed from a rhombohedral dodecahedron and a primitive, because in this way, the great circle Etna-Vesuvius-Mauna Loa, that of the volcanic axis of the Mediterranean and a third great circle parallelling the Andean chain constituted together a system of three rectangles. One such rectangle, orientated and placed using the methodology indicated above, is related to the regular pentagon encompassing nearly the whole of Europe and that is why Élie de Beaumont called it the European Pentagon (Fig. 35).

Élie de Beaumont's procedure of using eruptive centres as reference points for his pentagonal network of mountain range orientations may seem odd to us today, but do not let us forget that his theory of mountain-building, inherited in part from Leopold von Buch, included igneous intrusion and resultant uplift as a causative process of orogeny (Fig. 28B), although this was not easy to reconcile with his ideas on the contractional origin of mountain ranges.

Publication of his ideas on mountain-building brought instant international fame to Élie de Beaumont, because leading minds in the world of science approved of what he said. François Jean Dominique Arago (1830), a for example, remarked that until Élie de Beaumont's theory on dating the mountain ranges and on

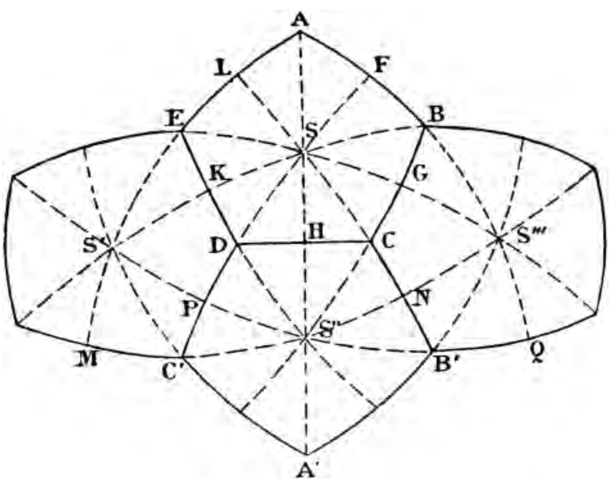

Figure 31: The system of trirectangles in Élie de Beaumont's *réseau pentagonal* (from de Lapparent, 1883, Fig. 605, p. 1241).

the relationship between the trend of a mountain range and its age was published, two geologists, like two augurs, could not face each other without laughing. Arago's article was promptly translated into German by Johann Christian Poggendorff (1796-1877) in his influential *Annalen der Physik und Chemie* (Arago, 1830b). Sir Henry de la Beche translated a shortened version of Élie de Beaumont's theory into English and arranged for its publication (Élie de Beaumont, 1831) in addition to incorporating the text into his *Manual of Geology* (de la Beche, 1831), which was to be translated both into German (de la Beche, 1832) and French (de la Beche, 1833) by no lesser authorities than Ernst Heinrich Karl von Dechen (1800-1889) and Élie de Beaumont's own teacher André-Jean-François Marie Brochant de Villiers, respectively; for the French edition, Brochant de Villiers asked Élie de Beaumont for a more detailed and updated text of his mountain-building theory and replaced it with the short English version with de la Beche's approval (de la Beche, 1833, pp. 616-665). Edmund Naumann, in his widely used and influential textbook *Lehrbuch der Geognosie*, introduced his readers to Élie de Beaumont's theory (Naumann, 1850, pp. 410-413; 1858, pp. 376-380) and Alexander von Humboldt cited it with enthusiasm by translating a letter Élie de Beaumont addressed to him on 15th December 1829 (von Humboldt, 1830) and then again with approval in his *Kosmos* (von Humboldt, 1858, pp. 578-579) quite forgetting his earlier just criticism of Cuvier's method of biostratigraphy (von Humboldt, 1823, pp. 41f), on which Élie de Beaumont's theory was heavily dependent. I must note, however, that most of these approving voices were for the timing and for the linearity of the mountain ranges (von Humboldt, however, expressly cites the *Notice*). When Élie de Beaumont came forward with the idea of the *réseau pentagonal* in 1850, he rapidly began losing followers. Naumann, for example wrote the following in the second edition of his widely read *Lehrbuch* (freely translated by me):

'That naturally with such a method every montain chain can be brought into some relation with any tesseral crystal form is a foregone conclusion. But, whether a natural law underlies this method applied with such brilliance and industry, or, whether the idea of this mysterious, not to say mystical, relation between the directions of mountain chains and the surfaces of imagined crsytal forms is something more than a glittering scientific phantasy, we may be allowed here to express a modest doubt.' (Naumann, 1858, p. 379).

In Europe, Élie de Beaumont's theory was cited along with Leopold von Buch's theory of magmatic uplift of mountain ranges and its part concerning terrestrial contraction received little attention for a long time. The 'uplift theory' as it was most commonly called and in which Élie de Beaumont's way of using unconformities allowed precise dating of timing of elevation, was applied to all mountains, even where one would least expect its applicability such as the Jura Mountains of Switzerland and France where not a single outcrop of magmatic or metamorphic rock can be seen.

[19] Regrettably Suess does not say what these 'new and sug-

gestive views on the origin of mountains' were. But they are not too difficult to guess. They are all in v. III of Élie de Beaumont's 1852 book: one can start with his distinction between those areas that are shortened (mountains) and those that are not (flat regions, as he calls them). The flat areas function as the two jaws of a vise. On this page 317, Élie de Beaumont even speaks of thrusting of one part over the other as a possible cause of the different elevations of the two parts (von Humboldt also cites this page in his *Kosmos*, v. IV {1858}, pp. 578-579). On the next page, Élie de Beaumont talks about crustal thickening and consequent uplift in a mountain chain. He does not consider the ideas of William Whiston (1696) and Sir John Herschel (in Babbage, 1837, notes F through I, pp. 182-217; 1838, notes F through I, pp. 204-247; see also Longwell, 1928) that came very close to spelling out the idea of isostasy. Instead, he implies that the interior of the earth is not compressible and therefore crustal thickening resulting from shortening must translate into uplift. On pp. 1327 and 1328 there is mention of gravity anomalies (he says deviations from the vertical of a plumb line) as a 'geognostical' tool (he gives credit to von Humboldt for that expression) to understand the deep structure of mountain ranges. Élie de Beaumont says that the gravity studies show that mountains are not mere heaps of material dumped upon the earth's crust, but that mountain-building effects the entire crust (a precursor to Airy isostasy! See Airy, 1855) . On p. 1330, he quotes Delesse saying that only the crystallisation of the interior of the earth must have resulted in a diminution of 1430 m of its radius. On p. 1333 he makes a distinction between absolute and relative uplift. Von Humboldt also quotes this page in the fourth volume of the Kosmos (p. 579). In the following pages Élie de Beaumont shows that the uplift of mountains is absolute, because the rise of a peak is larger than the subsidence occasioned by contraction.

A very important part of Élie de Beaumont's discussions in the third volume pertains to basins that form prior to mountain-building. He pointed out that within the area of semicircular 'slices' of a maximum width of some 2000 km. (these are the *fuseaux*=spindles, on account of their map shape; see Fig. 27F) on the face of the earth, small mountain chains commonly alternate with basins. Some of these basins were truly concave upwards (in Dufrénoy and Élie de Beaumont, 1848, p. 616, he had shown that basins with diameters not larger than 100 km could be concave upwards), but the big ones had to be convex upwards. Seas, gulfs, lakes, and river basins commonly occupy such basins (Élie de Beaumont, 1852, p. 1258) . To illustrate their overall character, he took one of the largest—and at the time the most popular, owing to von Humboldt's visit—the Pre-Caspian depression (von Humboldt, 1843, p. 311). He noted that the Pre-Caspian had a diameter of 841.044 m, and near Astrakhan (i. e., near its geographical centre), a depression of -24.75277 m. (Élie de Beumont was much given to mathematical precision, apparently without much regard to its significance). This depression, he calculated, caused a shortening along the diameters of one in two million! However, shortening was not uniform over the entire surface of the basin. Near the centre it was zero; it rose to a maximum towards the margins. Élie de Beaumont argued that this meant much of the shortening was concentrated in small areas and, in such areas, the ability of rocks to absorb the shortening without yielding would be overcome. So 'when the limit of yielding is reached in one place owing to the unequal distribution of the diminution of the surface imposed by the *bosselement*, the yielding material surges to the surface in the form of mountains...' (p. 1271). Here we have a description, much like the one he had given earlier of the Paris basin,

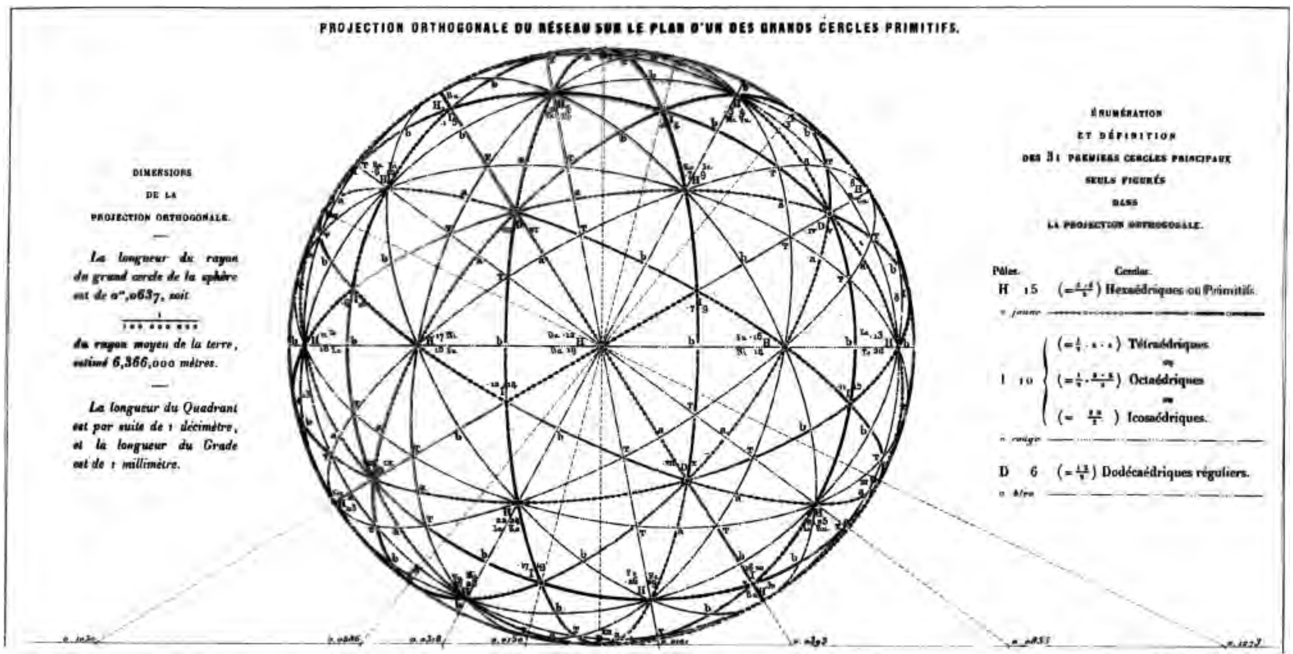

FIGURE 32: The complete *réseau pentagonal* (Élie de Beaumont, 1850, single sheet).

of first a depression forming, which then fails in a certain predestined place—where there is a maximum of shortening—to create a mountain chain. When we think that the American geologist Dana had at least known about Élie de Beaumont's book at the time he wrote his famous 1873 paper, we could perhaps better appreciate the roots of the idea of geosynclines and their relation to mountain-building in Dana's mind. Further, Élie de Beaumont was careful to point out that the *bosselements* left intact the visible fabric of the rocks affected: 'Such a feeble inflexion cannot dislocate, disrupt, or even tilt in a way perceptible to the eye, the sedimentary beds extending on the surface' (p. 1280). He criticised the common topographical and geological sections, vertically exaggerated many times, for giving a false impression of the extremely gentle structure of the *bosselements*; every basin drawn in such sections appeared to have steep margins, which gave the viewer the wrong feeling that they were fault-bounded. Such structures, if depicted at proper scale, would hardly be visible even on a model globe (Élie de Beaumont, 1852, p. 1314). That was one reason why it was so hard to recognise and to study them. Despite that, they were of immense importance. They showed the general mobility of the crust of the earth: 'These phenomena, large and little pronounced, less clear, less easy to grasp and to study than an unconformity of stratification or the structure of a mountain chain, but despite that appear very worthy of attention, as offering a proof of the general mobility of the crust of the earth and as an almost certain index to its thickness and to its flexibility' (Élie de Beaumont, 1852, pp. 1289-1290). Remarkably, Élie de Beaumont, the fine engineer, realised that flexure calculations could be used as a guide to the (elastic) thickness of the earth's crust!

Élie de Beaumont clearly separated the two major structural families of the earth's crust: 'Two different phenomena in the inflexions of the earth's crust are superposed and their effects mixed up, similar to the small ondulations on the surface of a liquid. On the one hand the general *bosselements* owing to the excess area of the crust, which are the causes of the new ridges into which the thin crust now and then contracts. On the other, the more or less pronounced curvatures of these ridges themselves, the formation of which accompanies the formation of the mountains.' (Élie de Beaumont, 1852, p. 1296).

With hindsight, we can see that Élie de Beaumont separated the large wavelength-small amplitude, reversible structures that generated no visible deformation on the outcrop and that represented the general mobility of the crust, from the small wavelength, irreversible structures that crushed and folded the rocks and that formed not continuously, but only now and then, when the yield strength of the rocks affected was attained. The amazing thing about this summary of Élie de Beaumont's views is that it would have been equally applicable to Hans Stille's views in 1960!

Suess praised the originality of Élie de Beaumont, but disagreed with many of these ideas. Suess' contracting earth did not have a crust (or lithosphere, a word which Suess preferred) behaving like a continuous plate buckling under end loads provided by the diminution of the volume of the globe caused by thermal contraction. His lithosphere was segmented, with different parts moving semi-independently of each other. This is the main difference between almost all other contractionists well into the second half of the twentieth century and Eduard Suess who chose to follow Constant Prévost's version of the contraction theory (Şengör, 2009b).

[20] Suess seems to be quoting verbatim from Sir Henry de la Beche's (1796-1855; Fig. 36) paper (de la Beche, 1846) here. He is not. He is in fact summarising a whole section that starts indeed on p. 221 and continues to the end of p. 227. This is the first part of the section entitled 'Undulations and contortions of the older rocks. Granites and elvans' of de la Beche's paper [this is typically a quartz porphyry seen in Cornwall]. Older faults' in de la Beche's long paper. In the part of this section dealing with the undulations and contortions of older rocks, Sir Henry indeed describes the dependence of the directions of folding postdating the deposition of the Old Red sandstone (Hercynian folds) on those that had formed before the Old Red was deposited (Caledonian folds). He repeatedly mentions 'lateral pressure' responsible for the formation of these folds and 'resistances' offered to the folding medium. I quote below those passages from Sir Henry's paper that seem to have made a lasting impression on Suess' mind:

'That there was any material disturbance of the rocks in the southern portion of our district, and indeed so far as surface evidence is afforded, on its eastern part, to the close of such of the coal measure deposits as are known to us, does not appear. From facts observed on the opposite districts of Ireland, the extent to which the older rocks of Wales may previously have suffered from contortion and violent movement remains to be ascertained. Part of Southern Wales seems to have partaken of the pressure which in South-Eastern Ireland is so well seen to have acted violently on the older fossiliferous rocks anterior to the deposit of the old red [*sic*!] sandstone and carboniferous [*sic*!] limestone of that country. The overlap of the old red sandstone, carboniferous limestone, and coal measures of Pembrokeshire, may be regarded as a modified prolongation of this movement, the effects of which became lost, as it were, in the direction of Herefordshire and Shropshire. Without the aid of the Irish sections, and these are as clear as could be desired, the value of the overlap in Pembrokeshire, as pointing to any great double and consecutive movements which the older rocks of Wales and Southern England may have experienced would scarcely have appeared. (de la Beche, 1846, p. 221).

......

..... The evidence of lateral pressure in these older movements is very considerable, so that if the multitude of contortions, domes, cavities, and flexures, into which the beds have been forced were flattened out, the area covered would be far more extensive than that now occupied by the same beds in their squeezed and crumpled state. How far there has always been displacement caused by the intrusion of igneous rocks, in a great measure concealed from surface exposure by more

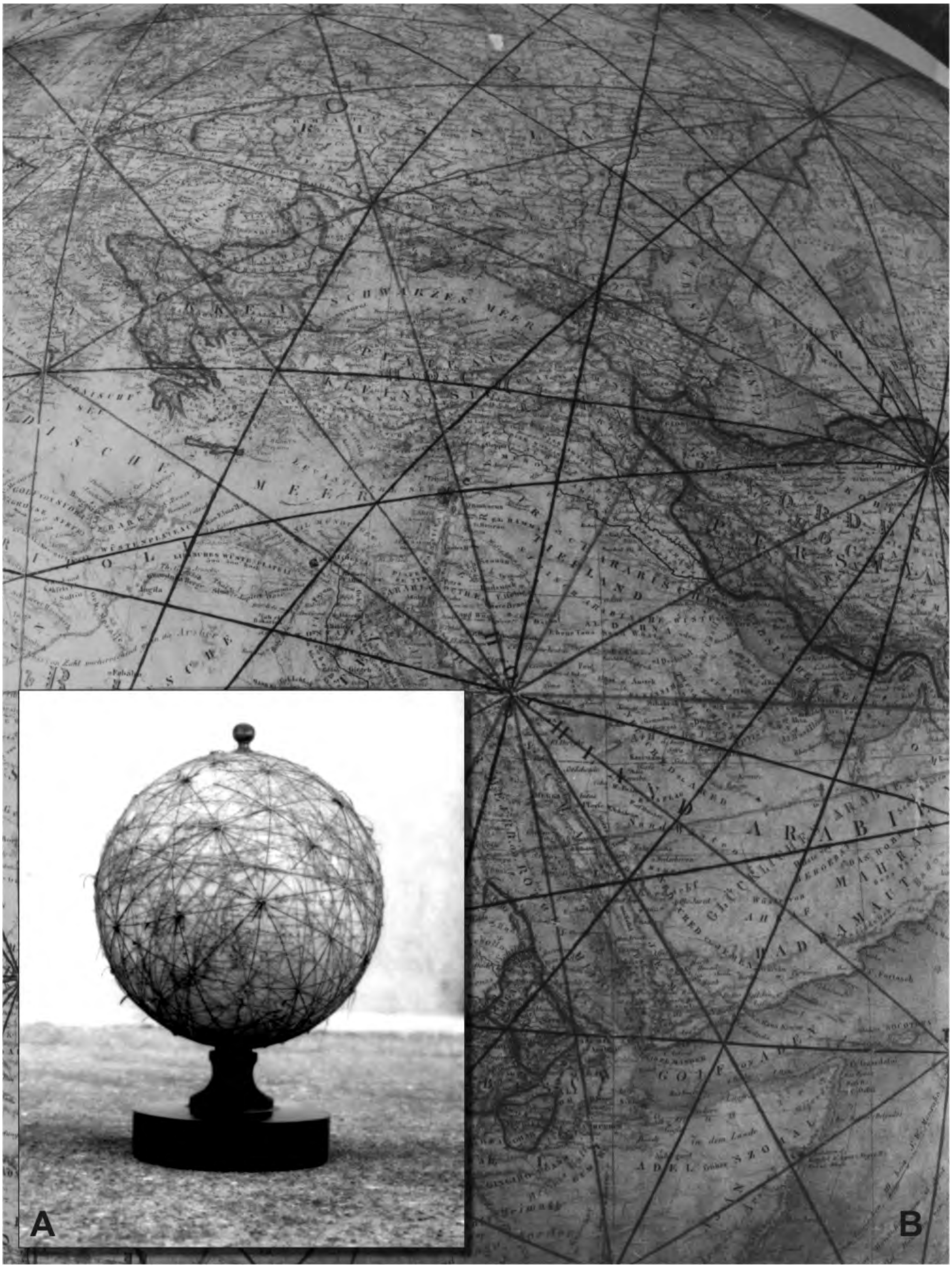

Figure 33: A: *Réseau pentagonal* placed over the terrestrial globe. Courtesy of the Collections of the École des Mines, Paris. B: Élie de Beaumont's 2m globe with the *réseau pentagonal* placed over it. Courtesy of the collections of the Collège de France. The globe is a Kiepert globe. The area shown reaches from Somalia to northern Germany and almost to northern Siberia and includes almost the entire Austro-Hungarian and the Ottoman Empires, two areas in which Suess' Viennese school was most active in field research. The western half of Suess' Altaids are also in view (see Şengör et al., this volume). In all these areas Suess and his students showed that Élie de Beaumont's *réseau pentagonal* did not have the remotest relationship to reality.

modern accumulations of various kinds, is a matter of research; but whether this explanation, or that supposing an adjustement of the surface of the globe at various times and places to a diminished volume of the earth from cooling, be the true one, lateral pressure seems evident from the singular folding and contortion of the beds, so frequently and so easily seen in many districts. (de la Beche, 1846, p. 222).

.....

The whole undulations of the old red sandstone, carboniferous limestone and coal measures, noticed as occurring in south Wales, in the adjoining English counties, and in Gloucestershire and Somerset, those in the latter counties especially having been long since pointed out by Dr. Buckland and Mr. Coneybeare[‡‡‡‡], more resemble adjustements to complicated pressure than to any prevailing lines of movement. As they are based on rocks which may have been crumpled before their deposit upon them, or ridged and furrowed in particular directions, some of these irregular movements may have been due to complications from very uneven resistances and pressure.' (de la Beche, 1846, pp. 225-226).

The idea that previously formed folds provide a hindrance to new fold formation seems to have been first expressed by the Rogers brothers in their epoch-making 1843 paper (Rogers and Rogers, 1843). The paper by Sir Henry de la Beche was the one that caught Suess' eye and must have appeared to him as a good explanation of the deflection of the Alpine-Carpathian arc south of the Bohemian Massif. It was this deflection by obstacles to the folding process and the change of trend of the fold axes caused by obstructions in the foreland of a mountain range that made him realise that Élie de Beaumont's theory that all mountain chains had straight trends and that such trends were also indicative of age must be wrong (Suess, 1875, p. 145). Finally, his own study of an anticlinal structure that goes from Geneva in the west to Wieliczka in the east published in his 1868 paper on the structure of the salt deposits in Wieliczka (Suess, 1868) convinced him that the Alps, like the Appalachians as described by the Rogers brothers, had been moved bodily northward with respect to undeformed Europe. He had also satisfied himself that the crystalline massifs in the Alps had no active part in mountain-building (Suess 1860a). Equipped with this knowledge, Suess looked at other European mountain chains and saw that they all had resulted from similar movements and their crystalline rocks were also passive participants in the horizontal movements. As a result of such observations and deductions, he had developed the never-published idea that there may have been an inhomogeneous movement of the entire lithosphere northwards 'towards the pole'. As soon as he looked at the Urals and Asia, however, he realised that this was not true and gave it up (Suess, 1875, pp. 145-146). By 1873, he had concluded that there was independent motion of bits of the lithosphere with respect to one another and that they had both shortening and extensional boundaries between them as we saw in his 'first guide' above.

[21] Note the emphasis on change step by step! Suess believed that science evolved step by step, slowly towards a better and better understanding of the universe. Every time he talks about the history of geology, this aspect was what he emphasised: in the *Entstehung* (Suess, 1875, pp. 145-146), in numerous passages in the *Antlitz* (especially in the opening chapter of the second volume, where he begins with Strabo and Dante: Suess, 1888, pp. 3-41), in his farewell address (Suess, 1902), in his foreword to the *Bau und Bild Österreichs* (Suess, 1903, pp. XXIII-XXIV). His entire lifework is an example of how he built his own edifice with material he collected from his predecessors, only arranging them differently in new conceptual buildings. He always praised the work of his predecessors and expressed his gratitude to them even where he criticised their ideas and even their persons severely (as we saw in note 16 above concerning Élie de Beaumont's 'childish behaviour'; see also de Novo y Chicharro, 1920, p. 15). Anybody who reads Suess' life work would be struck by how much it conforms to what his great countryman Karl Popper described as the way science in general functions by bold conjectures and merciless, observation-based refutations.

[22] This is a published book: see Sollas (1877[2010]). The British Library published a reprint of it in 2010 in its 'Historical Collection from the British Library' series.

[23] Suess lays emphasis on the fact that earthquakes are caused by tectonic events not only because he himself was one of the pioneers of this idea (Suess, 1873b, 1874), but because he used earthquake faults in the chapters of Part I of the *Antlitz* (Suess, 1883) to underline the uniformitarian character of tectonic phenomena, as he did the volcanoes to underline the uniformitarian character of igneous phenomena in his 'denudation series'. Immediately after he discussed the Deluge to show how trifling it was on a global scale in chapter 2, in chapter 3 he discusses 'some seismic areas' to underline that earthquakes are caused by faults. Then, in the fourth chapter he discusses the kinds of dislocations. He then moves on, in the fifth chapter, to igneous phenomena and introduces the denudation series: from volcanoes to subvolcanic features and then finally to giant batholiths (the word batholith was invented by Suess and first introduced in this chapter). Finally, he closes the part I of the *Antlitz* by a classification of earthquakes according to the type of movement they occasion and reviews the denudation series. The reader gets a good lesson on how to use present phenomena to decipher their effects in the geological record.

[24] The theory of eveation craters supposed that volcanic mountains, especially the basaltic ones, result not from accumulation of ejected material, but from uplift about the chimney. This idea was first developed by Leopold von Buch in the Auvergne in the Massif Central when he finally abandoned his teacher Werner's neptunism. In his tour to the volcanic districts of Central France in the April of 1802, while in Neuchâtel on an official visit from

[‡‡‡‡] Here Sir Henry adds the following footnote: 'Observations on the south-Western Coal measures of England, Geol. Trans., Second Series, vol. i. In the map accompanying his memoir many anticlinal lines are laid down.'

the Prussian government, Leopold von Buch, who was at the time still a neptunist, not only became convinced that basalt could also be a volcanic rock (he was not yet convinced that *all* basalts were volcanic), but at the same time conceived the idea that the rock that he called domite—oligoclase-bearing hornblende- and biotite-trachyte—occurred, in the Puy de Dôme and in other cones, as giant blisters propelled upwards by 'internal volcanic power' (von Buch, 1809[1867], p. 483). Mont-Dore was a further surprise for him, since he could see on this 'volcano' neither a crater nor lava flows resembling those familiar to him from the Vesuvius. A uniform cover of basalt lay over multifarious porphyries. Because von Buch had come to acknowledge that basalt had been once molten, he developed the peculiar idea that basalts first must have formed in molten lakes, then solidified into flat layers and were only then uplifted—clearly a neptunist hang-up of a recent convert to volcanism! The 'circus' on top of Mont-Dore, he interpreted as an extensional collapse structure, not as a crater (von Buch, 1809[§§§§][1867], p. 513ff.). His visit to the Canaries in 1815 convinced von Buch completely that basaltic islands were not volcanoes—not in the ordinary sense anyway:

'With the overview of this remarkable, outstretching island [he means La Palma, or, more accurately, San Miguel de la Palma, in the Canaries], with the view of the size and the depth of the central cauldron [Fig. 37], with the thought that here not lava flows, but beds rise uniformly from the sea to the highest point, one can almost see the whole island rising from the bottom of the sea. The beds were raised by the uplifting agent, the elastic powers of the interior and in the middle these vapours broke out and opened the interior. This crater would then be a result of the elevation of the island and that is why I call it a crater of elevation in order never to confuse it with the craters of eruption, via which the volcanoes communicate with the atmosphere. Even the wonderful barancos [*sic*!], which dissect the slopes in such an incredible number, appear to be a direct result of this elevation. They are true cracks across the outer periphery of the beds; ... Water flows in them only in those short intervals when there is snow

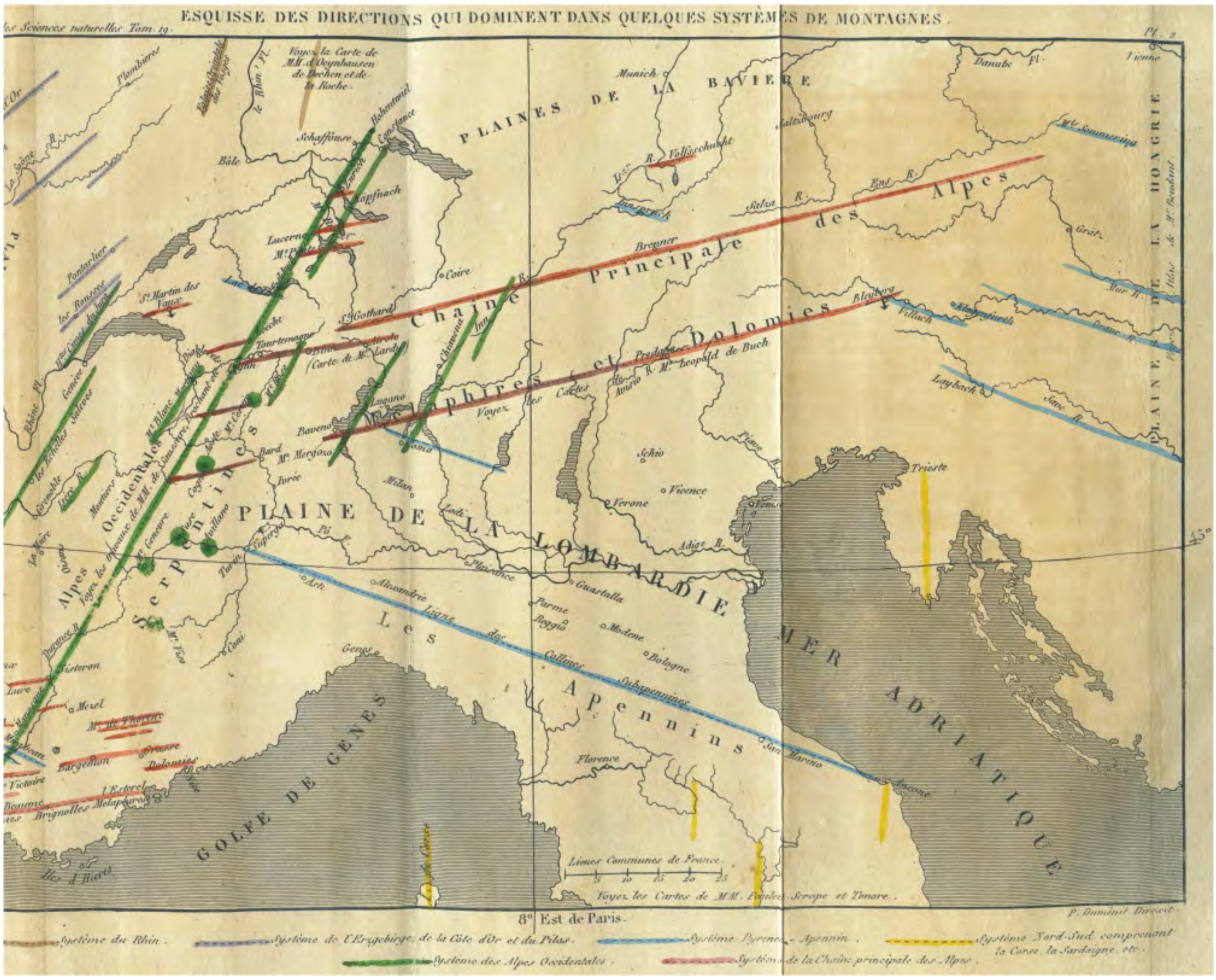

FIGURE 34: The Alps according to Élie de Beaumont (1829-1830, plate 2). Every trend is coloured differently and differently-coloured trends supposedly originated at different times. Suess' global survey showed that this scheme had no basis in reality and undermined all such simplistic and regularistic opinions in tectonics. The Kober-Stilleans revived some of them in the twentieh century (see Şengör, 1982b).

[§§§§] This book was actually published in 1806. The 1809 date is a printer's error.

on the mountains. One cannot ascribe to such waters the origin of these valleys, as even the strongest stream cannot dissect firm rocks like a knife. The beds were elevated towards the middle. So, they must tear and leave cracks behind, because the same inextensible material must distribute itself across a larger area on the surface of a cone. We see the same effect, when we suddenly and strongly push upwards a firm piece of clay." (von Buch, 1820[1877], pp. 9-10).

From this inference, von Buch moved, in 1824, to the uplift of whole mountains by igneous intrusions (accompanied by metamorphosing gases and liquids) from below. The Alps were for him a show case (von Buch, 1824[1877]). Élie de Beaumont became his loyal disciple both in his interpretation of volcanoes and mountain ranges (see especially his paper summarising von Buch's ideas on elevation craters and elevation of mountains including the latter's four plates of alleged elevation craters containing the view of La Palma after his acquarell {Fig. 17 herein}: Élie de Beaumont, 1830b; Élie de Beaumont added the component of horizontal shortening resulting from planetary contraction).

Although the issue of elevation craters remained contentious, there was general agreement among geologists, before the first half of the nineteenth century closed, that most of the relief of the earth was due to uplifts and subsidences governed by the internal processes of the planet. One dissonant voice, that of Constant Prévost (1787-1856, Fig. 38), disturbed this complacent picture and prepared not only the ground for the theory of eustatic movements, to be developed by Eduard Suess in 1888, but, by profoundly influencing Suess' overall thinking in tectonics, became a critical catalyst in the generation of the grandest tectonic synthesis of the earth before the rise of plate tectonics in the sixties of the twentieth century.

Doubts began to form in Prévost's mind about what he had learnt from his teachers Cuvier and Brongniart concerning the Tertiary stratigraphy of the Paris Basin, when he found in 1821 mixed faunas of marine and freshwater environments (see, Şengör, 2009a). These separate faunas had been earlier used by Cuvier and Brongniart (1811) as proofs for successive revolutions of the surface of the globe separating distinct time periods in the history of the earth (Prévost, 1821). Prévost's continuing studies revealed in the subsequent few weeks that the mixture was much greater than could be dismissed as a local accident (Prévost, 1822). He became convinced that there had been no repeated deluge catastrophes and subsequent retreats of the sea. He thought that the sea had retired only once, and the mixture of faunas had been a consequence of subaerial erosion, transport, and sedimentation (see Suess' take from this development: Suess, 1888, p. 17).

Prevost's conviction of 'the retreat of the sea once' gradually led him to an Anaximandrian view of the historical geology of the earth, assuming that it had been characterised by a progressive regression of the seas from the continents. It was mainly this idea that eventually led him to deny the possibility of any primary uplifts of the earth's crust and thus to pave the way for Suess. But, for that idea to reach fruition, Prevost's involvement in the debates on the elevation craters had to intervene.

When the so-called Islet of Julia (Graham island as commonly known in the English-language literature; Ferdinandea of the Italians) suddenly made its appearance in the strait of Sicily in the July of 1831 (see Dean, 1980; Şengör, 2009a), Prévost was charged by the French Academy of Sciences with the mission of studying the phenomenon on the spot. Although in his first letter to the president of the Academy he stated, after only a cursory look, that 'around the island of Julia there must be a belt of uplifted rocks, which would be the rim of an elevation crater,' (Prévost, 1831), he later claimed that, in his first report, he had been circumspect and said that nothing in the structure of the new island indicated that it was created by an uplift of the ground.

Prévost's major memoir summarising his findings came out in 1835 and it embodies an all out attack on the theory of craters of elevation. He pointed out that not only the new islet of Julia, but neither Vesuvius, nor Etna, nor indeed Stromboli and Vulcano, which he had opportunity to see first-hand (Prévost, 1835, p. 120), showed any evidence of uplift. They were structures of accumulation and the radial valleys emanating from the crater rims were products of fluvial erosion. In this he was thus in complete agreement with Scrope and Lyell (see, Prévost, 1835, p. 124). He later visited the groups of Cantal and Mont-Dore, which Dufrénoy and Élie de Beaumont (1834) had already interpreted as elevation craters, to take a look for himself. He found them perfectly comparable with the other volcanoes he had come to know and summarised his conclusions in eight items in a letter to the president of the Academy of Sciences, appended to his report on the Islet of Julia (Prévost, 1835, pp. 121-124), concluding:

'The most attentive and impartial examination has led me to see, in the groups of Mont-Dore, Cantal and Mézenc, only three volcanoes formed like the Vesuvius, and even better, like the Etna, by the succesive accumulation of volcanic materials, erupted from numerous orifices in the form of flows, or pulverised and fragmentary projectiles.

My trip in Auvergne has gone to confirm the ideas that had been formed in my mind by my study of the volcanic terrains of Sicily and Italy and convinced me especially that products of volcanism only locally, nay, even rarely, dislocate the ground across which they reach the surface. The Tertiary terrains of the Limagne and of the surroundings of Clermont, those of the basin of the Puy, the granites which surround the red rock, furnish proof that the most violent eruptions of scoriae and cinders, the most abundant eruptions of trachytes, of basalts and lavas could take place, amidst terrains of diverse nature, without producing any noticeable disruption.' (Prévost, 1835, pp. 123-124).

With these words, Prévost not only denied the existence of elevation craters, but further affirmed that the most violent magmatism may take place without creating any noticeable deformation of the ground. The latter observation was a direct negation of the claim by von Buch (1824), that mountain chains were raised by the upwelling of augite porphry and quite catastrophically (von Buch, 1827).

Were mountain chains really 'raised?' Was anything in the rocky rind of the earth really 'raised?' In the thirties of the nineteenth century Constant Prévost came to believe that geomorphological and/or structural features that are commonly thought of having been raised, were in fact products of subsidence. In a published discussion remark he stated (in the third person as recorded during the discussion) in 1839:

'One is very much inclined to go back to the movements of the ground and especially to uplifts to explain geological facts. He [i. e., Constant Prévost] did not doubt that the ground had experienced large-scale and intense dislocations many times. He thought, however, that large subsidences were the main events and uplifts always occupied a very limited place. In order to emphasise his ideas he entered into a discussion of the theory of so-called *uplifts*, which he believed, should be more properly called the theory of *subsidences*, if one wished to express with a single word the cause of the changes in the relief of the ground. The cause that changes the relief of the ground was nothing but the shrinking and the contraction of the consolidated crust of the earth, which it experiences as a consequence of its continuous cooling, and not, as some believe, a fluid or a gas agent tears up hindrances on its way

Figure 35: The European pentagon in a gnomonic projection on the plane of its centre (from Élie de Beaumont, 1852, v. III, unnumbered foldout plate 3). The small black circles are worm holes in my copy.

and uplifts them while attempting to escape from the interior of the earth. The granites, the porphyries, the basalts come out from the ground, as lavas do, by exploiting the fissures of the crust dislocated during the contraction. To ascribe to these materials the dislocations themselves is nothing but a confusion of cause and effect.' (Prévost, 1839, italics his).

In 1840, in a lengthy reply to Rozet to defend this opinion, Prévost began defining what he understood by upheaval:

'*Upheaval* of the ground means, according to me, the raising of this ground above its original level by an uplifting force, that is to say, applied under it and working from the inside towards the outside of the terrestrial sphere.

The *theory of upheavals* is therefore that, which consists in creating reliefs on the surface of the earth such as volcanoes, mountain chains, plateaux, inclination and verticality of strata, faults, etc. by the *uplift* of the masses forming the ground by means of an agent placed under the consolidated exterior of the earth which pushes out this resistant part, *uplifts*, *deforms* and *splits* the dislocated panels' (Prévost, 1840, p. 184, italics his).

Uplift as expressed by the theory of elevation was impossible, thought Prévost, simply because his stratigraphic studies had shown him that earth history had been characterised by a continuous, irreversible sea-level drop. He rightly pointed out that if elevations occurred within the existing ocean basins, this would diminish their capacity leading to overflowing and therefore to sea-level rise. Since he denied that any sea-level rise had ever occurred, he was inescapably driven to assume that only subsidences were possible. At this point, his experience with volcanoes and the so-called elevation craters came to his aid. Had they not shown him that all claimed uplifts had in fact turned out not to have been uplifts, but accumulations; that magmatic rocks had been seen not to have caused any appreciable deformation of the basement which they had traversed? Prévost concluded that subsidence alone was sufficient to give rise, by 'rebound' (he says 'par contre-coup'; only in 1850 it turned out that by 'contre-coup' he meant folding of the subsided areas as they are forced into narrower spaces), to local absolute elevations, lateral shortenings, bendings, foldings, ruptures, squeezings, faults, etc. (1840, p. 186). He viewed magmatic rocks only as passive, rising wherever they could find an opening or some other opportune condition, thus preparing the ground for Suess (1875), who finally laid to rest the hypothesis of the active rôle of magmatic intrusions in mountain-building nearly four decades later.

FIGURE 36: Sir Henry de la Beche.

All of this kind of deformation was a consequence of the thermal contraction of the globe: 'I believe very simply, as do nearly all geologists of our day, that the terrestrial spheroid is a cooling body. The consolidated outer crust floats on a still fluid or soft interior. The body loses volume in such a way that the exterior, while trying to keep pace with this centripetal motion of the interior tends to get folded, undulated, broken, engulfed etc. By analogy, I think that the same cause produces in numerous phases the folds, the undulations, the ruptures, the depressions which constitute the present surface features of the earth.' (Prévost, 1840, p. 201).

Prévost's ideas had to incubate for another thirty-five years to hatch in Eduard Suess' *Die Entstehung der Alpen*. The offspring of this union was the greatest tectonic synthesis of the globe the world had ever seen: Suess' *Das Antlitz der Erde*. All of this had begun by the attack on the craters of elevation, by Scrope, Lyell and Prévost. Suess wrote:

'While Beaumont [*sic*! Élie de Beaumont is meant] had replaced the word 'elevation' with the phrase 'ridge-building', his brilliant opponent Prévost denied expressly and with certainty the presence of any centripetal*****, uplifting force. According to Prévost, the uplifts were only a secondary consequence of the subsidences, as claimed by Deluc [*sic*!]††††† before him. A

***** This is a slip of Suess' hand. It is clear that he meant centrifugal.

††††† Jean André de Luc (1727–1817). Swiss naturalist. He was a good observer but allowed his views to be influenced by his fervent religiousness that pushed him into a reactionary position against the developments of the geology of his time. Suess here means the following place in De Luc's publication (de Luc, 1779, pp. 467-468). Because de Luc's book is not easy to find, I cite the passage here to underline the one concept that seems to have greatly impressed Suess via Prévost (italicised passages express that concept):

'1. The sea covered our continents in the past and it no longer does so. 2. At the same time continents existed that seem no longer to exist. 3. The sea covers a bed, which is stable and no causes are known to give us the possibility of destroying this bed or building a new bed. *Therefore, a change in one part of the bed of the sea must result from one definite cause, determined by a local event.* 4. *The cataclysm brought about by this new state must affect all our continents at the same time*, where the soil has the same thickness. 5. This bed has only a very small thickness seen from the viewpoint of the events that caused it.' The critical point was here that a local tectonic event in the sea must influence the entire ocean. This is the key Suess used with which to understand how a Cuvierian stratigraphic record may be explained by using a Lyellian geological theory of only local events.

great step had already been taken by the first publications of Prévost on this subject, such, for example, as the report on the Isle of Julia of 1832 in that, the volcanoes were relegated to a position of subordinate side effects, in the sense of the idea, victoriously defended by Scrope and Lyell, that there are no "elevation" craters' (Suess, 1875, p. 3).

[25] This structure had long been thought to be a temple dedicated to the Graeco-Egyptian god Serapis devised on the order of Ptolemy I, one of the diadochi, with a view to unifying the Greeks and the Egyptians in his kingdom (*Tempio di Serapide* or simply *Serapeo*). Its excavation had started in 1750 in Pozzuoli (Roman Puteoli, perhaps a continuation of the Greek colony of Dikearkheia founded in the 6th century BCE). However, it has since turned out that instead it was a *Macellum*, i. e., a fish and meat market, vindicating a view by the noted numismatist, antiquarian, diplomatist and civil servant Mr. Francesco Carelli [1758-1832], the perpetual secretary of the *Accademia Ercolana*, in his *Dissertazione Esergetica sulla Sagra Architettura degli Antichi*, cited by Lyell (1830, p. 453). The *Macellum* had been built in the beginning of the second century and began sinking under the sea in the third. In the sixteenth century its floor was already five metres under the sea level. In the eighteenth century it again had emerged completely, but covered by marine sediments. In the beginning of the nineteenth century it started sinking again (d'Antonio, 2003, pp. 1121-115).

It was the evidence afforded by the *Serapeo* that had given Lyell the confidence to defend the theory of vertical continental oscillations (Lyell, 1830, pp. 449-459). Suess, however, used the evidence provided by Issel (1883, especially map facing p. 177) and others to examine the case of the *Serapeo* and concluded that it was simply a case of local up- and down-movement caused by the filling and emptying of an underyling magma chamber, although he was careful to point out that these filling and emptying episodes did not correlate with the eruption history of the Vesuvius or any other volcano around the Bay of Naples (Suess, 1888, chapter 9, pp. 463-499 and coloured plate VIII). Here time proved Suess right (for some of the recent studies on this phenomenon, see Dvorak and Berrino, 1991; Orsi et al., 1999; Battaglia et al., 2006; Bodnar et al., 2007).

[26] In *Die Entstehung de Alpen* Suess interpreted the secular rise of Scandinavia, well-documented since the days of Urban Hiärne (1702, 1706), as a folding phenomenon:

'Under these circumstances one should imagine that the true movement of Scandinavia has the shape of a very stretched fold with the short concave opposite fold in the south or that a horizontal line from the German shore over the Nordkap would turn into a ∽ in a given time. But then it is not necessary to assume the presence of a force different from that of mountain-building ... The unusual distribution of certain marine deposits, as I think I was able to show on the example of the medial Cretaceous, cannot be explained even by the most extensive level changes alone.' (Suess, 1875, p. 151).

By 1888 he evidently thought even that interpretation was too much of a consession to the uplift theory. In 1885 from June to September he had been in Norway with his physician friend Dr. L. Burgerstein checking out the terraces claimed to be the witnesses of former higher sea-levels (see Suess, 1916, chapter XXII, pp. 365-372). He had quickly convinced himself that the terraces all the way to Tromsö (Fig. 40) had nothing to do with high sea-levels but were witnesses of the former levels of the lakes which ice blocking episodically turned a fjord into.

They then journeyed farther south and southeast. An expert on the region, geologist, polar explorer and customs officer, at the time the administrator of the Tromsö Museum (and one its founders), Karl Johan Pettersen (1826-1890), reviewed the terrain with them suggesting itineraries. They selected two points south of the triple junction formed by the Russian (now Finnish)/Norwegian/Swedish frontiers between 68° and 69° N latitude. The first itinerary led through the Dividal to the water divide north of Lake Old Vand and the second through the Sördal to the large Lake of Torneo on the other side of the water divide (Fig. 40).

The *seter* (or *setär*) were like numerous benches cut into

FIGURE 37: Leopold von Buch's Aquarell of the 'elevation crater' of the island of San Miguel de la Palma in the Canaries. From the collections of the Museum für Naturkunde, Berlin. The view, from the south-west, shows the inside of the island, the original 'caldera', the Caldera de Taburiente. The caldera has been a national park since 1954.

FIGURE 38: Constant Prévost.

bedrock. Neither their number, nor their elevations correlated from one fjord to another. Suess noticed that they become numerous and higher towards the heads of the fjords. Seters turn around and in places join terminal moraines. As far as he could see from the existing maps, and an aneroid he had brought with him, they were also horizontal. Nowehere did Suess see any marine fossils on them and neither had any-body else before him (Suess, 1888, p. 430). They thus could not have been cut by the waves of the ice-age ocean. Suess thought that they could not be marine terraces, but erosional terraces of once existing lakes between the bedrock and the moraines containing them seaward (for the cross-sectional geometry of such lakes, see Flint, 1971, fig. 13-11).

Lower down were genuine marine terraces, but Suess ascribed them to sea-level that he thought stood higher during the ice ages: 'The terraces met with in very open bays, as for instance in Christiania [i.e., Oslo] fjord, which is essentially different from the narrow fjords of the north, may indeed be genuine vestiges of a sea-coast, like the terraces of western Patagonia, which also occupy an open situation; but the seter are not so, nor are many of the terraces of the west coast of Norway, especially those of considerable altitude.' (Suess, 1888, pp. 458-459). That sea-level was higher during the ice ages in the northern latitudes than it is now was a widely held view during Suess' time and was ascribed to the gravitational attraction of the ice masses that had gathered around the North Pole. The rise of sea-level along the border of an ice cap of 38° angular radius and a central thickness of 3300 metres had been estimated at from 40 to 175 metres (Geikie, 1903, p. 378 and the references given in footnote 4 there). Thus, Suess' assumption was not only not unreasonable, but was perfectly along the lines of the best of the orthodox thinking of his day. Today, rise of sea-level along the periphery of an ice cap because of the gravitational pull of the ice mass is well-known, corroborating Suess' inference (Clark et al., 2002; Bamber et al., 2009).

Figure 39: The 'Temple' of Serapis, actually the *Macellum* of ancient Puteoli (now Pozzuoli). Notice the black to dark grey discoloration of the columns. They are caused by the *Gastrochaenolites* created by *Lithophaga*, i.e. the 'rock-eating' date mussels. They mark former sea-level.

The richness of Suess' observations in northern Norway, the ingenuity displayed by him in interpreting them with the aid of a vast array of both comparative and theoretical arguments about the terraces of northern, western and southern Scandinavia, are truely awesome. His arguments range from fluvial and glacial geomorphology, through climatology to hydraulic engineering. When the reader reaches the end of the eighth chapter, he or she feels that all that Suess now would need to do is to turn the corner of Scandia and to take his reader into the Baltic Sea and into the Gulf of Bothnia to complete the argument against any Quaternary rise of Scandinavia. Instead the reader is taken abruptly, in the ninth chapter, from the icy north to the sunny Mediterranean, to consider the evidence offered by the Temple of Serapis in the Gulf of Pozzuoli, west of Naples.

For all his ingenuity and boundless knowledge, great ability as a field geologist, we know now that Suess' interpretation of Scandinavia was wrong. He thought that the terraces around the Bothnian Gulf were made by a still emptying inlet, thus almost going back to the ideas of Anders Celsius (1701-1744; see Celsius, 1744). Nothing shows better than this chapter the aim of *Das Antlitz der Erde* as a theoretical long argument against the uplift theory.

[27] The sentence 'the undeniable fact that the terminology of formations, created in England, might be applied over the whole globe' is the key to understand why the palaeontologist and stratigrapher Suess ended up becoming the greatest tectonician who ever lived. If we consider Tuzo Wilson the Einstein of tectonics because of his invention of plate tectonics, then surely Suess is its Newton. What motivated Newton was the strange observation that planetary orbits were ellipses as shown by Kepler, not circles as had been supposed since antiquity and by Galileo in defiance of Kepler's results. He assumed a fixed time and fixed space and solved the problem of gravitation. Enstein showed that Newton's fixism was not warranted. What motivated Suess was the unexpected observation that sedimentary sequences on all continents were correlatable. He solved the problem of understanding this peculiar property of the earth by reversing an old assumption, namely the fixity of the sea-level. He showed that Leopold von Buch's and Lyell's assumption of a fixed sea-level was not warranted, and introduced what he called the eustatic movements, and everything he did in tectonics followed from that. Wilson simply showed that fixed continents that Suess assumed were not necessary for the succees of his tectonic models.

But let us first see how Suess formulated the problem taking his sentence quoted above from his letter to Sollas as our guide:

'Supposing our listener to have now reached the point, so that he stands on the threshold of stratigraphical geology, and at the same time of the history of life: he will find himself surrounded by an overwhelming mass of details concerning the distribution, stratification, lithological character, technical utility, and organic remains of each subdivision of the stratified series. He stops to ask the question: what is a geological formation?[‡‡‡‡‡] What conditions determine its beginning and its end? How is it to be explained that the very earliest of them all, the Silurian formation[§§§§§], recurs in parts of the earth so widely removed from one another—from Lake Ladoga to the Argentine Andes, and from Arctic America to Australia— always attended by such characteristic features, and how does it happen that particular horizons of various ages may be compared with or distinguished from other horizons over such large areas, that in fact these stratigraphical subdivisions extend over the whole globe?' (Suess, 1883, p. 10).

Notice here that Suess takes the *systems* to be *natural* subdivisions of the stratigraphic record and views them as packages of rock laid down uniformly over the entire globe. This is the old, Wernerian idea of 'universal formations,' as recognised by his friend and admirer, the great genius Marcel Bertrand in the preface he wrote to the French translation of the *Antlitz* and Bertrand says 'the same alternations of movements and similar deposits of ancient seas are found from the plains of the United States to Russia; all of this had been ignored or hardly supposed; all of this is today classic and incontensted' (Bertrand, 1897, p. XIV).

Suess was baffled by this, because as a Lyellian, he realised that this could hardly be possible on an uniformitarian earth where all geological phenomena were supposed to be local. He voices the question he must have asked himsel in the beginning of his own career:

'This question is certainly obvious and justifiable, but if we could assemble in one brilliant tribunal the most famous masters of our science, and could place this question of the student before them, I doubt whether the reply could be unanimous, nay, I do not even know if it would be definite. Certain it is that in the course of the last few decades the answer would not always have been the same.' (Suess, 1883, p. 10).

While writing these lines, he must have been thinking not

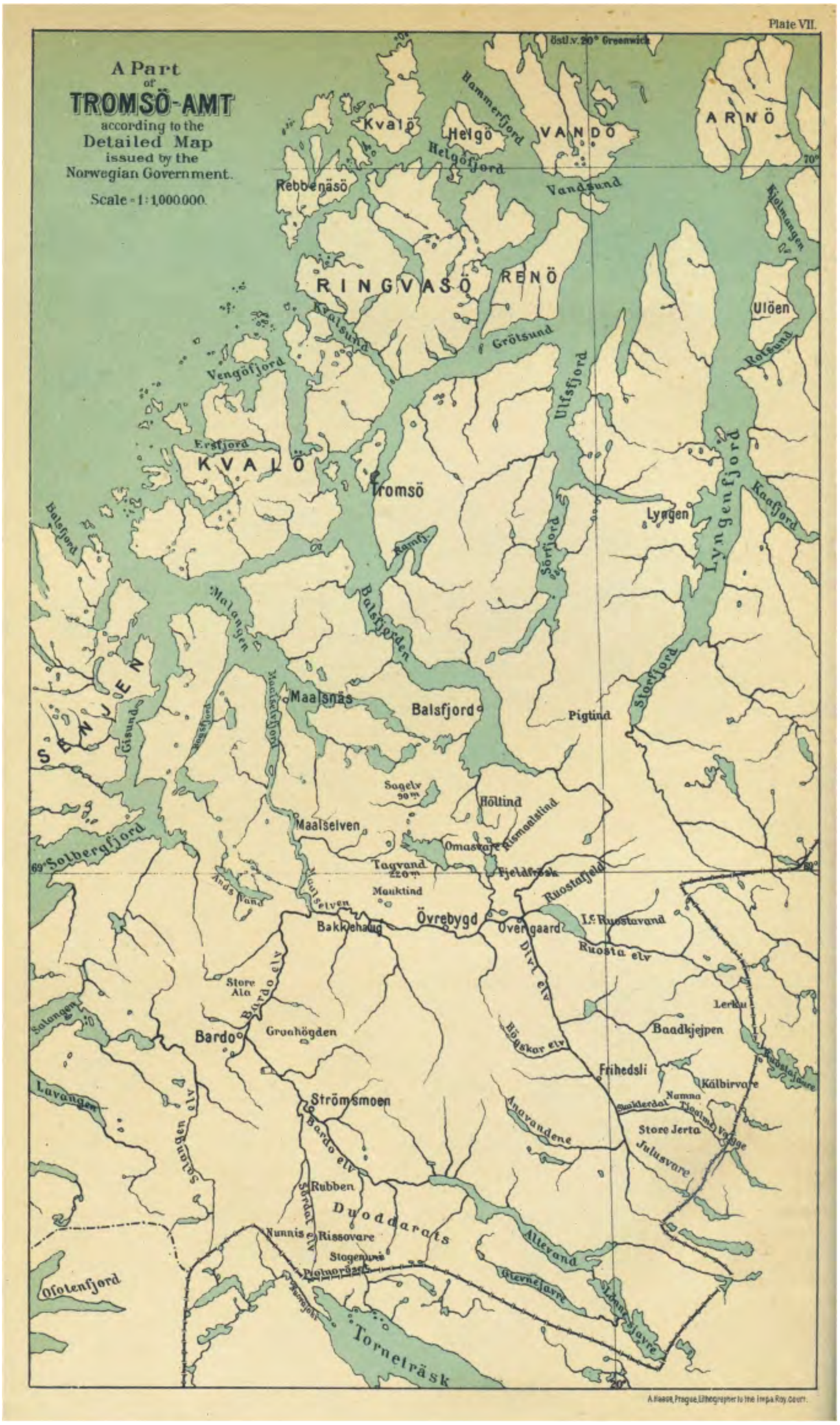

Figure 40: Map of the area explored by Suess personally to check the nature of the water-cut terraces in northern Norway in the summer of 1885. This is plate VII in the second volume of the *Antlitz* (Suess, 1888), here copied from the English edition (Suess, 1906).

[‡‡‡‡‡] While reading this entire quotation from Suess it is of utmost importance to bear in mind that when he writes 'formation,' he means a 'system' in our present stratigraphical usage. For its present usage, see Salvador (1994, pp. 81-82).

[§§§§§] About the usage of Silurian here, see note 5 in this section, but Suess definitely uses the term here in its Murchisonian sense, i.e., as one that includes the Cambrian, Ordovician and Silurian proper.

only of the Cambrian-Silurian dispute between Murchison and Sedgwick (Secord, 1986), or the Devonian dispute between those two and Sir Henry de la Beche (Rudwick, 1985), or the continuing slicing of the Tertiary into ever narrower subdivisions (Suess, 1883, p. 12), but also of his own experience in which the Rhaetian was considered by the French geologists (and by himself for a long time) a part of the Lias, whereas it was thought a part of the Triassic by the German geologists.

After having stated the question and the uncertainties attached to the various answers given, he reviews, in the *Antlitz*, the evolution of ideas that finally culminated in the statements by such geologists as d'Archiac, Hébert in France and Beyrich in Germany that it was not mountain-building, i.e. not orogeny, that determined the end of a formation, as Élie de Beaumont believed, but it was the slow up and down motions of the continents. For a time Suess went along with this idea. So, to explain the transition from marine to non-marine Miocene deposits in the Vienna Basin he had not hesitated to assume uplift—an uplift that was Europe-wide (Suess, 1862, p. 54). But as this work continued, especially near Eggenburg in the outer Vienna Basin, i.e., in a part of the Alpine Molasse Basin, he also began corresponding with geologists working on similar deposits elsewhere. It was, however, the Russian geologist Nicolai Pavlovich Barbot de Marny's (1829?-1877) letters describing the Neogene sections north of the Black Sea and even near the Aral Sea that shocked him. Barbot de Marny was describing the same beds and at the same elevations from places thousands of kilometres away. He finally decided that no continental uplift could be that perfect to maintain the same level across distances that were measured in thousands of kilometres. He thought it had to be the sea-level that was moving. That is why in his *Erinnerungen* he says that the idea of eustatic movements was born in the field around Eggenburg (Suess, 1916, p. 139). In the Erinnerungen he wrote:

‚During such wanderings in the plains exhibiting long-stretched zones of Mediterranean deposits maintaining a constant height on the slopes of the old rocks before me and filled with the idea that a similar thing occurs in the wide Hungarian plain, I was first possessed by the idea that such extensive evenness could not be brought about by raising the land, but only by depressing the sea-level.

This idea bit deep into the foundation of the prevailing geological views, but many factors invited a closer inspection, especially the fact that many larger islands rising up high from the waves of the ocean carry an animal population and a plant cover identical or very closely related to those of the nearest continent, so that one would like to see them as parts of these continents. These insular terrestrial faunas and floras could not possibly have been raised up from the depth of the sea; but a change in the level of the sea surface could cut them off and leave them as relicts.

First the facts, such as relations of elevation, the fossil shells, etc. had to be followed as closely as possible. For that Eggenburg offered a convenient opportunity. But only fifteen years later, after I had learnt more about the distribution, did I dare to pronounce this opinion publicly.' (Suess, 1916, pp. 138-139).

The fifteen-year-delayed publica-

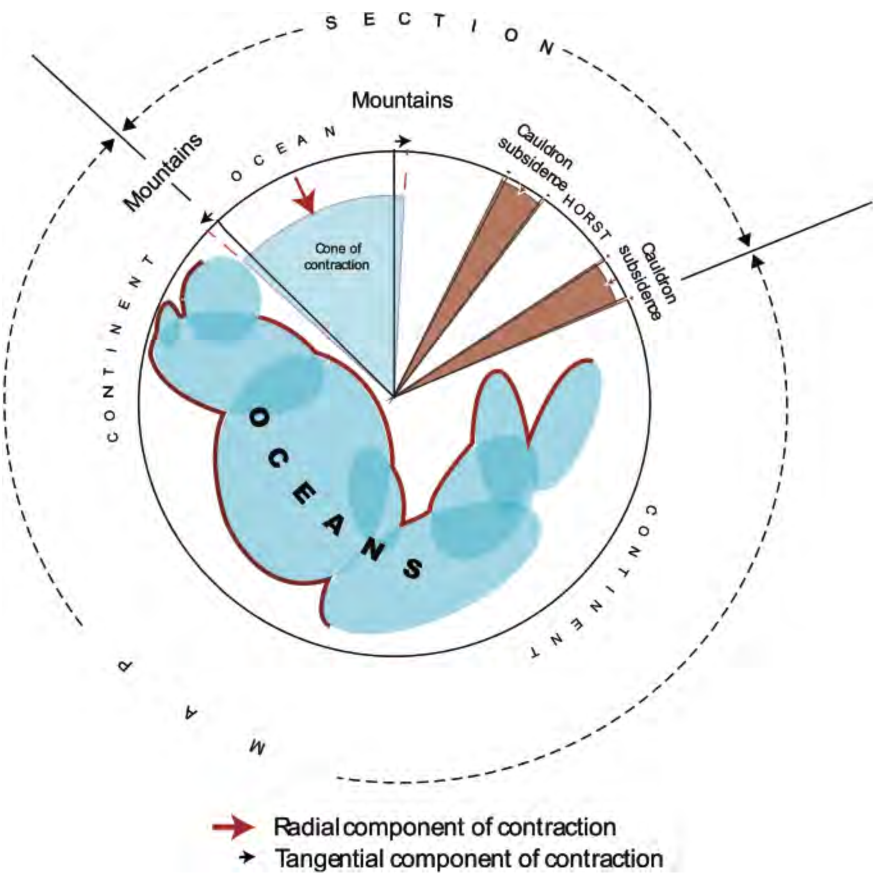

Figure 41: Eduard Suess' model of terrestrial tectonism, reconstructed from his descriptions in his two fundamental publications, *Die Entstehung der Alpen* (Suess, 1875) and *Das Antlitz der Erde* (Suess, 1883-1909). Suess divided the effects of the thermal contraction of the earth into a radial component and a tangential component. The radial component was supposed to be expressed by cauldron subsidences, essentially cone-shaped volumes of the earth with apices in the centre of the planet. As these sectors contracted in unequal amounts, their bases on the surface of the planet subsided differentially in shapes approximating irregular ellipses. When many such adjacent elliptical areas subsided, their coalescence formed ocean basins. In the figure, the 'section' shows cross-sections of contracting volumes. The 'map' illustrates how intersecting elliptical subsidences may form oceans. I drew them in such a way as to represent the southern and central Atlantic and the Indian Oceans, although Suess never illustrated such specific examples. His database was simply insufficient. The only purpose of this figure is to make his theory intelligible to the reader and to show that it was at the time a plausible idea to entertain. From Şengör and Atayman (2009, fig. 1, reproduced with the kind permission of the Geological Society of America, Inc.).

tion here referred to is Suess (1880).

But Suess was working around Eggenburg in 1860 and 1861 (Suess, 1916, pp. 137-139) and published his local results in 1866 (Suess, 1866). Darwin's theory had not yet been published and he was still a Cuvierian in his palaeontology and in his stratigraphy. After *The Origin of Species* was published in 1859, nothing was the same again and this remarkable book brought its own serious problems into stratigraphy. In the *Antlitz*, Suess continues his discussion of the problems of stratigraphers with a confession by Darwin:

'At this point, Darwin's book on the Origin of Species made its appearance:

"But just in proportion as this process of extermination had acted on an enormous scale," says the author, "so must the number of intermediate varieties, which have formerly existed on the earth, be truly enormous. Why then is not every geological formation and every stratum full of such intermediate links? Geology assuredly does not reveal any such finely graduated organic chain, and this perhaps is the most obvious and gravest objection which can be urged against my theory. The explanation lies, as I believe, in the extreme imperfection of the geological record."

In a later passage Darwin says:

"I believe that the world has recently felt one of these *great cycles* of change; and that on this view, combined with modification through natural selection, a multitude of facts in the present distribution both of the same and of allied forms of life can be explained."

These words, although applying only to the geographical distribution of forms of life at the present day, contain nevertheless the important admission, that the development of life has been, according to Darwin, *uninterrupted, but by no means uniform*; nay, it almost appears as though the reader were to be introduced to a further problem, that of a great and as yet unknown rhythm in the evolution of living beings—a rhythm dependent on episodic changes in the external conditions of existence.' (Suess, 1883, pp.12-13, emphasis by Suess).

Suess at this point cites Aristotle from his *Meteorologica* (Μετεωρολογικά) and gives away what his suspicion is as to the causes of the abrupt changes in the biota:

'Aristotle seems to be alluding to the same problem in the remarkable passage: 'The distribution of land and sea in certain regions is not always the same, but that becomes sea which once was land, and that land which once was sea, and there is reason to believe that this change take place according to a definite system and at definite intervals of time."' (Suess, 1883, p. 13)******

After this small—but extremely significant—digression into antiquity, which will reveal its full significance in the whole rest of *Das Antlitz der Erde*, Suess returns to the palaeontological and stratigraphic records:

'More than twenty years have passed since the publication of Darwin's book. Since then observations have multiplied; we are now able with much greater certainty to trace the descendence lines of organisms among the relicts of the past.....

The continuity of life is thus more and more clearly illustrated by the results of palaeontology; yet the fact remains that we do not find species varying gradually within the limits of single families or genera, and at different times, but that whole groups, whole populations and floras, or if I may so express myself, complete economic unities of Nature appear together and together disappear. This is the more remarkable, as the transformations effected by in the populations of the sea and in those of the land by no means invariably coincide: a fact which has been proved in the most convincing manner by a study of the various subdivisions of the Tertiary formation in the Vienna Basin. From this we may conclude with certainty that the determining factors in this case have been changes in the external conditions of life.

It is true that the record is extremely incomplete. A certain proof of this lies in the local recurrence of some groups. The recurrence of certain species of ammonites in the Jurassic system of Central Europe has already been made use of by Neumayr to determine, in their main outlines, the boundaries between the zoological provinces which existed during the several subdivisions of the Jurassic period††††††. Communications have from time to time been established between these

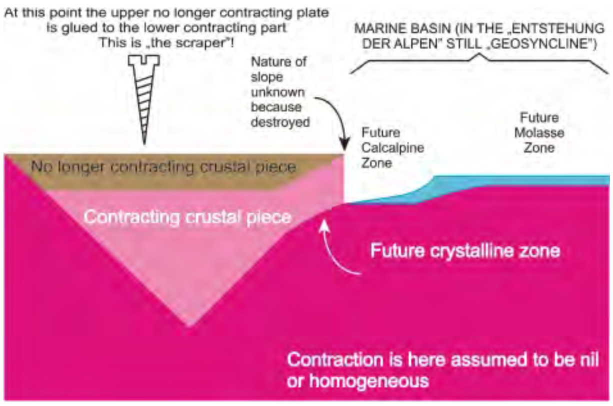

FIGURE 42: The most puzzling aspect of Suess' theory orogeny for his contemporaries was that it required that while the front of a mountain is being shortened, extension characterise its hinter side and that this be done on an inhomogeneously (he wrote 'unequally') contracting planet. People thought such a model needed a hand from the outer space (*ex-coelo* Löwl had written) to come down to push the independently-moving blocks. This and the next figure show that Suess' model of orogeny was kinematically possible given three conditions: 1) That contraction be inhomogeneous. He often said it was. 2) That there be different depths of deformation, that in essence there be displacement gradients downward into the earth along which displacement may change abruptly (décollement) or gradually. This too he wrote about. 3) That at some point not in the centroid of the contracting area there be a point or area of no displacement gradient (essentially a zone of attachment of the upper non-contracting and lower contracting layers). The screw shown in this Figure shows where, in the particular case here illustrated, that point (or area) of attachment will be.

****** Suess cites Aristotle as Aristot. Meteor., XII in his endnote 22. This reference is completely wrong: the passage he cites—and he cites its contents correctly— occurs in book I, chapter XIV, paragraph 351a19. For a detailed discussion of Aristotle's theory, see Şengör (2003, pp. 42-46.)

†††††† Although Suess here cites no specific reference, he had in mind Neumayrs *Jurastudien*, especially numbers 3 and 4 (Neumayr, 1871).

provinces and again suppressed; yet not only may the synchronism of the subdivisions in one province and another be determined in many cases with certainty, in spite of subsidiary differences, but throughout the whole earth we see the well-known general type of the Jurassic formation succeeded by the equally well-known type of the Cretaceous; and from this we may conclude that changes must have occurred which have exerted an influence over an area still more extensive than that of these great provinces.

On this fact depends the unity of stratigraphical terminology. The excellent work of English geologists in East Australia, the reports of the Geological Survey in India, the accounts of our explorers in China and in the Arctic regions, the voluminous publications presented to us by North America, as well as the works of German investigators on the Andes of South America, the descriptions of the Cape, and the scantier but most valuable accounts which we have received from the less easily accessible parts of Africa all these works, when they wish to designate the more important parts of a stratified series, make unhesitating use of terms which were originally chosen to describe the classification of the deposits in a limited portion of Europe. When it is a question of marine deposits the geologist in New Zealand or Victoria knows as well as his colleague in North Russia or Spitzbergen whether he has Palaeozoic, Mesozoic, or still younger deposits before him, and expressions such as "Carboniferous Limestone," " Jurassic," " Cretaceous" have now become naturalized in all parts of the world visited by geologists. (Suess, 1883, pp. 14-15)

Reading the above passages one would be inclined to believe that Suess was a naïve layer-cake stratigrapher. But he immediately counters any such suspicion in the next paragraph:

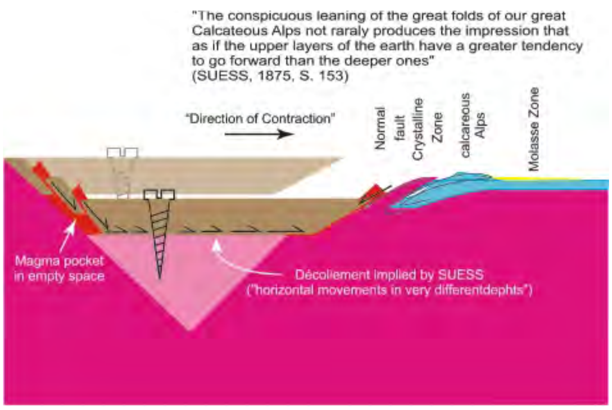

FIGURE 43: Here the pale pink aera has contracted. The upper, non-contracting plate has moved forward towards the right (in the Alpine case to the north) and pulled away from the left (in the Alps from the south). So, shortening occured in front of the mountain chain and extension took place at the back. The extension created empty spaces that could serve as conduits for magma to rise to the surface creating vulcanicity. Here it is assumed that the non-contracting plate is not at all shortened, so it subsided as a whole except the bits in the back. This need not be the case. Instead of the foreland, the overiding plate may be shortened, or both. This would give rise to mountain-formation. Dipping detachments could also have the same effect. Suess' cricits evidently did not think hard about what he was saying. They attacked him on points at which his theory was not assailable. And he knew it.

'The greater part of this nomenclature originated in England, and has obtained general recognition in spite of the fact that certain vast marine deposits occur in Central Europe, the chronological equivalents of which in England bear an entirely different character, and are not immediately recognizable. Such are the Triassic formations of the Eastern Alps, and the Tithonian series. At the same time Abich, in his works on Armenia, and Waagen and Griesbach in India, are making known to us marine faunas, by which the mighty gap occurring in Europe towards the close of the Palaeozoic period is being steadily filled up. But more careful consideration easily convinces us that it is not the completeness of the series of marine formations in South-east and Central England, but rather, so to speak, that mean frequency of gaps among them, which has facilitated the conception of natural groups, in a manner which would never have been suggested by other places where one marine deposit regularly follows another. In those districts, however, where the incompleteness of the series is particularly great, and where, for example, the encroachment of the Cenomanian is apparent, there is a most striking correspondence over large areas and in both hemispheres. It was this correspondence which led me long ago to suppose that the so-called secular elevations and depressions of continents are not sufficient to explain the more limited distribution of some and the wider distribution of other formations, a phenomenon of which the cause, though unknown, must be general.' (Suess, 1883, p. 15)

This is an amazing insight, which many a biostratigrapher of our day has yet to appreciate! Suess says that precise stratigraphic correlation cannot be done on a palaeontological basis alone. One needs a physical phenomenon that affects the entire globe to cause the gaps in the record everywhere. These we can correlate. The palaeontologist and the stratigrapher Suess is here after a tectonic process that might help him with his global correlations. One wonders whether he had taken a hint from von Humbodt's great insight that Cuvier's biostratigraphy was hard to apply globally because of the geographical barriers separating entire communities of organisms (von Humboldt, 1823, p. 41f.). In any case, we realise today how right Suess was, when we look at the usage we make of magnetostratigraphy and chemostratigraphy and how much more easily boundary problems are resolved by using magnetic or isotopic signatures than using only palaeontology.

Suess continued:

'In like manner E. v. Mojsisovics has since designated as one of the most remarkable phenomena known to us "the precisely parallel development of the chief features of the two great continental masses of the Northern hemisphere," and the "concordant cycle of dynamic transformations on both sides of the ocean."

Some years earlier eminent American geologists, following quite another course of observation, thought that they recognized, within the stratified series of their continent, a certain periodic return of those conditions under which the deposition of sediments takes place. According to them every great formation begins with a deposit of clay or sand in shallow water, and is then followed by a calcareous marine deposit, whereupon the depth of the sea again diminishes.

These series were spoken of as 'cycles of deposition.'

.....

But if we accept these conclusions, and consider the detailed succession of a sedimentary series in its dependence on the great laws which govern the conditions of deposit, then the mode of stratification, and even each individual stratum, gain, as terms of a great rhythmical process, an importance hitherto not attributed to them [Here I feel like writing: welcome to cyclostratigraphy].

That reservation which I made, with regard to the chronology of terrestrial faunas, holds good in so far as an alteration in the terrestrial fauna is not necessarily contemporaneous with an alteration in the marine fauna; but the great works of Marsh [1831-1899] and Cope [1840-1897] show already clearly enough to what a large extent the Tertiary land faunas of North America correspond to those of Europe. This fact is particularly instructive, since it demonstrates, even more forcibly than a consideration of the marine deposits, the simultaneous appearance and disappearance over vast areas of whole communities, of whole economic unities; the same phenomenon which Heer [1809-1883] long ago happily designated "the periodical recoinage of organisms."[‡‡‡‡‡‡]

In Europe the great local variations in the characters of the deposits which furnish the remains of terrestrial animals far more marked in this continent than in the United States obliges the investigator to trust almost exclusively, certainly to a far greater extent than in the case of marine deposits, to organic remains for his chronological and stratigraphical conclusions. But it is scarcely necessary to remark that, while the characters of the terrestrial fauna afford a most valuable passive criterion, yet it is the physical causes of faunal transformations which will, when once they are recognized, form the only true basis for a delimitation of chronological periods.' (Suess, 1883, pp. 15-17).

What were these physical phenomena that would enable us to erect a reliable global stratigraphy? Suess answers it in the next paragraphs, which are the last before he tells his reader of the plan of his book. The reader gets the feeling that the whole book was written, because its author felt that he had solved the one great and central problem of geology: precise correlation of rocks the world over that keep the record of the history of the earth.

'These physical causes are probably very different in kind. As our brief account of the fluctuation of opinion in the last decennium or so has shown, the cause of the changes in the organic world has been sought mainly in movements of the earth's crust. The advance which has been made in our knowledge of the structure of the great mountain chains does not, however, bring us any nearer an understanding of this supposed connexion. The manner in which contraction of the earth's crust manifests itself on the surface of the planet, in the formation of folds and faults, does not accord with the hypothesis of moving continental masses, which, over wide areas, repeatedly ascend and descend in a slow and uniform manner. The similar development of the sedimentary series on both sides of the Atlantic Ocean, and the correspondence in the gaps of this series, cannot be explained in this way. When, in some of the best accounts of the structure of a mountain chain, we find, side by side with an exposition of the formation of the folds and fractures, an appeal made to supposed "elevations in mass," independent of the formation of the mountain chain,

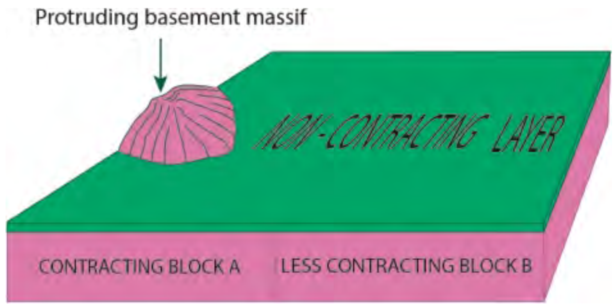

Figure 44: This and the next three figures ilustrate how Suess' orogeny functioned in four dimensions. In this figure a non-contracting layer is underlain by two different blocks, contracting differentially. Block A has a protruding massif that will function as a buttress to shortening later.

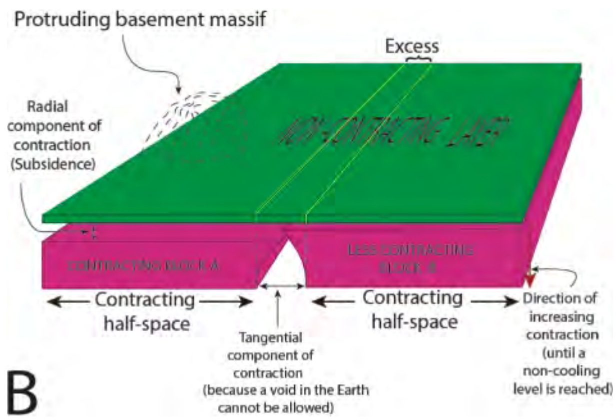

Figure 45: Here both underlying blocks have contracted, but done so in different amounts. Notice that contraction increases with depth, because those volumes lose more heat. Not only their tops subside differentially ('radial component of contraction'), but a space originates between them that must be closed by shortening ('tangential component of contraction') as no empty spaces at depth can be allowed because of gravity.

‡‡‡‡‡‡ Again here Suess cites no specific reference, but he most likely had in mind Oswald Heer's last chapter in his great book, *Die Urwelt der Schweiz* (=The Primordial World of Switzerland; Heer, 1865, see especially p. 601 where he says 'that in a relatively short time a *recoinage of forms* takes place and that the newly coined species stays unchanged for thousands of years. *The time of persistence of species in a certain form must be much longer than the time of the coinage of the species.*' The emphasis is Heer's. This is one of the numerous early recognitions of the idea reinvented and popularised most recently by Gould and Eldredge, 1977, under the designation 'punctuated equilibrium'.) Heer is said by his former student C. Schröter in his memorial to his teacher, published in the *Neue Zürcher Zeitung*, 16th-18th October 1883, to have used the term '*Ausprägung*' (=coinage) for the first time in the third volume of his *Die Tertiärflora der Schweiz*, 1858, v. 3, p. 256.

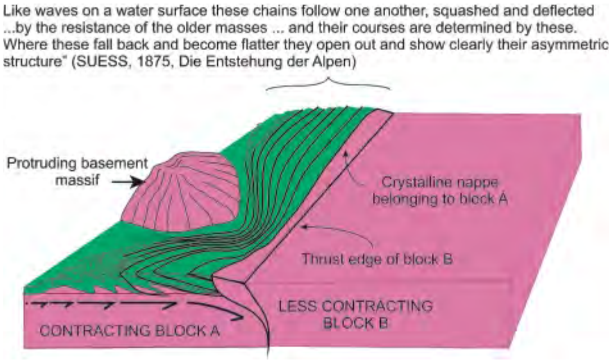

Figure 46: The collapse of the space between the blocks A and B leads to shortening at the surface. Because the block A subsided more than block B, the cover of block is removed by erosion, but the cover of block A is shortened to accomodate itself on its basement that became smaller in area. The protruding basement massif hinders the forward march of the folds creating a virgation. The crystalline nappes belonging to block A formed from the lips above the space between the blocks A and B in the previous figure, formed because of increasing contraction at depth.

to account for some incompleteness of the series, it cannot fail to strike us that this is an assumption at variance with the rest of the explanation. We are left with the impression that phenomena essentially different in character have not been sufficiently distinguished from one another.

Let us consider the contrast between Élie de Beaumont's conception of the limit of a formation, and the views on which Beyrich based his classification of the middle Tertiary deposits. This contrast finds its clearest expression when to the term *dislocation* we oppose that of *transgression*.

A dislocation, whether it consists in folding or faulting, is limited to a definite mountain system, often even to a very small part of it; a transgression extends over a large part of the earth's surface. The intensity of a dislocation is subject to very rapid local variations; in a transgression, difference of intensity, within the limits of a single region, can hardly be distinguished, and a transgression may often extend over large areas in complete concordance with the underlying beds. The dislocated stratum was already in existence before the occurrence of that event, into the nature of which we are about to inquire; the transgressive stratum was formed after or during that event. Investigations into the structure of great mountain chains have in recent times given us a continually deepening insight into the causes of dislocations; the question of transgressions still forms the subject of opposing theories.

That dislocations are the result of true movements, that is, of relative displacement of various parts of the planet, needs no explanation, the word itself expresses it. But this is not true of transgressions, nor has the word been chosen with this intent.

Under various forms the theory has long been maintained that along with the movements of the earth's crust, changes take place in the form of the surface of the sea. The remarkable extension of certain transgressions leads us to return to this view. A close investigation of the most recent events, such as are indicated by ancient shore-lines situated above the existing sea-level, can alone lead to definite results. But even a hasty consideration of such strand-lines suffices to show their complete and absolute independence of the geological structure of the coast. In Italy the lines of former sea-levels are met with on the various promontories of the Apennines in undisturbed horizontality, here on limestone, there on the ancient rocks of Calabria, here once more on the ash cone of Etna. The complete absence of any relation between the ancient shore-lines and the structure of the mountains may be proved by hundreds of examples. But the supposition of a uniform elevation or depression of a continent, so complicated, and divided into so many fragments, without any mutual displacement of the parts, a supposition necessary to explain the horizontal course of these lines on the separate portions of a mountain complex, cannot be brought into harmony with our present knowledge of the structure of the mountains themselves. Thus this circumstance, too, leads us to infer independent movements of the sea, that is to say, changes in the form of the hydrosphere.' (Suess, 1883, pp. 17-19).

Suess finally brought us to the eustatic movements, although in 1883 they had not yet been named. It is now clear why he prefers Prévost's theory of contraction rather than Élie de Beaumont's. It enables him to change the capacity of the ocean basins at will and do it in a way so as not to violate Lyell's actualism and his localism. Yet the way Prévost enabled him to change the volume of the ocean also made it possible for Suess to make the ocean to leave a global record as Cuvier demanded. This record could be pretty abrupt, because it was easy to lay bare the shelves and to reinundate them with only small changes in the level of the ocean. Every time shelf areas disappear, the most fertile regions of the biosphere, vast communities of animals and plants become extinct. Every time the shelves get reflooded, evolution takes an explosive step. Here was finally a *realis causa*, a *verum causa* for the so baffling transgressions and regressions and the accompanying extinctions. Suess must have felt elated when he discovered this.

We also now see how the brilliant insight he had into the essence of mountain-building in 1873 could be bogged down in the contraction theory. Prévost's contraction worked so well

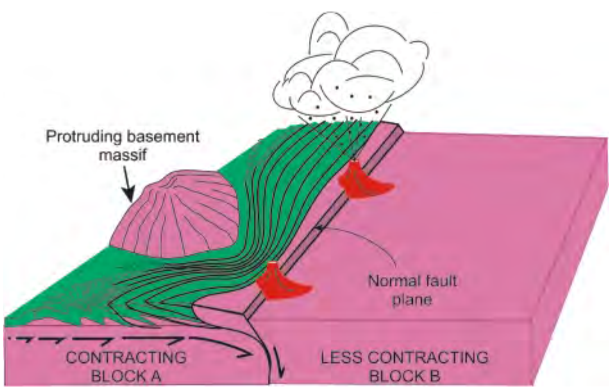

Figure 47: Finally, as the block A continues to contract, it subsides and creates normal faulting with volcanoes on top according to the mechanism outlined in Figs. 42 and 43, but not shown here (it is supposed to take place at a deeper level here).

for the most enigmatic problem in geology, namely the transgressions and regressions and the punctuated evolution of life, that Suess must have thought that it had to be true. Also, ist application to orogeny, although problematic, was not insurmountably so and Suess offered brilliant models to get around the problems contraction heaped in front of him while coming to grips with mountain-building. When he wrote, 'The collapse up of the terrestrial globe, this it is we witness' ('Der Zusammenbruch des Erdballes ist es, dem wir beiwohnen': Suess, 1885, p. 778), this is what he meant (Fig. 41). As the earth collapsed into itself, it created new oceanic spaces into which the waters retreated giving rise to regressions. As the volume of the earth diminished it also gave rise to shortening in its upper parts. To accomplish that, Suess had to assume different depths of detachments of crustal flakes and different sites of their attachment to their underground to enable them overthrust their forelands and extend their hinterlands.

This was the crucial difference in interpreting orogeny between Suess and all of his contractionist successors, except J. Tuzo Wilson (and predecessors, for that matter, with the exception of Prévost). In his popular text-book, Ferdinand Löwl (1856-1908) wrote, for example (freely translated by me): 'According to Suess, the advanced part of the mountain arc experiences the strongest overfolding and overthrusting, while the concave back side, from which the one-sided push originated [Suess never said that the push originated from the back side! This is Löwl's assumption based on other, common contractionist models, which he carried over to Suess' model without further ado, obviously because he paid no attention to Suess' description of different levels of detachment under the moving flakes], is torn by a stretching force and magma is allowed to rise through fissures. This idea was conceived first on the basis of the positions of the Carpathian and Tyrrhenian sites of eruption, but came out of the unusual assumption that a southerly stretched block could create a northerly shortening. Under these circumstances it is understandable that Suess attempted never to document his idea scientifically, but tried always to illustrate it metaphorically [this is totaly untrue as the quotations and illustrations given herein document!]. The most characteristic picture he found in a paper that appeared in 1879 [*sic*!] 'Die Heilquellen Böhmens' [=the medicinal springs of Bohemia]. It is stated there that he "could imagine in no better way the origin of a great mountain range than to imagine his hand be wounded through scraping thereby the skin would be thrown into folds on one side and it would tear on the other and some blood would well up. So we see a major mountain range always shortened on one side and thrown into folds; on the other side it would be stretched and torn and where it is torn volcanoes appear from the inside of the earth." This picture is well chosen so far as it makes it immeadiately apparent that the origin of the one-sided push cannot be in the crust, as the origin of the scraping cannot be in the skin. Only a push *ex coelo* [from the heavens] could generate the necessary stresses to cause forepush in the convex side and tearing on the concave side.' (Löwl, 1906, p. 173).

Having thus criticised Suess' theory of orogeny, Löwl could find no way out other than a wholesale return to Élie de Beaumont's theory: 'If one abandons the untenable and, on top of all, entirely unnecessary hypothesis of the one-sided push, the mountain chains appear as weak crustal strips pressed, as if between the jaws of a vise, between stronger blocks as a consequence of the compression of the outermost shell of the globe of the earth, as in the old and entirely apposite idea of Beaumont [*sic*!].' (Löwl, 1906, p. 173).

It is shocking how many prominent geologists and geographers of the Dark Intermezzo in the twentieth century approved of Löwl's ill-conceived critique (e. g., Supan, 1908, pp. 626-627; Tietze, 1917, p. 447; Stille, 1924, p. 277; Haarmann, 1930, S. 13; Bucher, 1933, p. 259) and indeed returned to Élie de Beaumont's version of the contraction theory, usually using Dana's presentation in his *Manual* and in his much-cited 1873 paper, although Suess' theory corresponds to the observations better. None of these authors could imagine how what Suess described could be accomplished by contraction. The main reason for this was that Suess never provided theoretical diagrams to illustrate his views. Had he done that, the problem may have never surfaced. That is one reason why I have chosen to make this guide to his tectonic thinking as richly illustrated as possible. Without diagrams, understanding Suess' tectonics requires years of careful reading and thinking and consultation of his sources as I know from personal experience.

Figs. 42 and 43 illustrate the manner Suess may have imagined how simultanious shortening in front and extension behind a mountain belt could occur *using only his descriptions*. Figs. 44 through 47 illustrate how Suess' orogeny indeed creates mountain ranges very similar to those that result from plate tectonic processes and why they were so different from that of all other contractionists except perhaps that of Constant Prévost, but we can never know what Prévost really

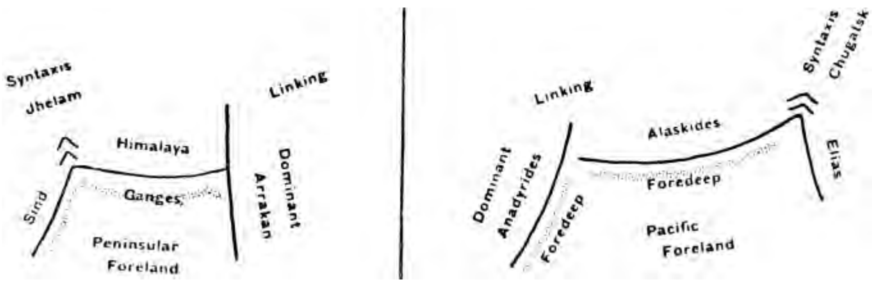

Figure 48: Comparison of the syntaxes and linkings north of India and north of the Pacific Ocean. From the unnumbered foldout plate in the fifth volume of the English edition of Suess' *Antlitz* (Suess, 1924). These sketches exist only in the English edition, because Sollas asked Suess to send them to him to clarify some of the concepts discussed in the *Antlitz*.

thought about the detailed geometry and kinematics of orogeny, because he left us no detailed discription of his ideas about it (only in a discussion of Élie de Beaumont's paper he simply implies that as a large area subsides, its top, i.e., the non-contracting layer, would be folded, would show a *contre-coup*: Prévost, 1850, p. 444, footnote 1). In the twentieth century the only contractionist who followed Suess' steps was Tuzo Wilson, but after 1960 he stopped being a contractionist and sometime between 1960 and 1963, he invented plate tectonics.

[28] That there are two kinds of continental margins was one of Suess' great discoveries. Some of his fixist contractionist successors tried to do away with it (e.g. Stille, 1957), although they are now an indispensible part of the plate tectonic theory of geology accounted for in terms of rifted (Atlantic-type) and subduction (Pacific-type) margins. It also has become customary to speak of passive (Atlantic-type) and active (Pacific-type) continental margins, but these appellations are inappropriate because both margin types are active, but show different kinds of activity: whereas Pacific-type margins participate in subduction phenomena, Atlantic-type margins become extended first and then subside thermally creating the immense shelf deposits we are familiar with from mountain belts. Major earthquakes, up to and perhaps a little beyond Mw 7, do take place along the Atlantic-type margins. Plate tectonics has added a third type of margin, namely that of transform fault (California-type) margin (Dickinson, 1976).

Suess first seems to have recognised the distinction between margins along which active mountain belts exist with the ocean floor as their foreland, separated from them by foredeeps and those at which the fabric of the continent is truncated by faults, with any mountain ranges that happened to be parallel with them being not genetically related to them, in the first volume of the *Antlitz*, when he was reviewing the geology of the French and the Iberian and north African Atlantic margins:

'The parts of the west coast of Europe enumerated above differ widely in tectonic importance. The patches in the Cotentin, the deposits of the lower Loire, as well as those of the Tagus, and those along the Portuguese coast around Cape St. Vincent, we consider to be parts of the old Atlantic coast or of the Atlantic sea-floor. The depressions of the Gironde and the Guadalquivir, on the other hand, correspond to the course of two great mountain chains and conceal the junction between these and their foreland. However great may be the significance of the Straits of Gibraltar as regards the existing physical geography of our continent, yet they do not enter into the scheme of its general structure, and from this point of view may be regarded merely as an accident of quite subordinate interest.' (Suess, 1885, p. 376).

Here is the beginning recognition that the Guadalquivir depression had something to do with the mountains of the Betic Cordillera, but not the Atlantic margins north of it. That there is a difference between the margins of the Pacific and the Atlantic is spelled out in the last chapter of the first volume, but its discussion is relegated to a later, 'special' chapter:

'The preceding remarks furnish us with a basis for the consideration of that fundamental difference which exists between the outlines of the Pacific and of the Atlantic Oceans. Several portions of these coasts have already been discussed in the foregoing sections: we have pointed out the remarkable similarity both in structure and stratified sequence which is presented by all the Coastal Cordilleras, from Staten Island and Cape Horn onwards to the north, through Patagonia, Chili, Peru, and the tropical region of South America, and again through lower[§§§§§§] and upper California*******, i. e., through more than a quarter of the earth's circumference. After having described the manner in which the syntaxial arcs of the great chains push forward against the north of the Indian peninsula, we observed that a similar advance of syntaxial arcs takes place towards the north of the Pacific Ocean, and that a special tectonic homology exists between that fragment of ancient table-land and this part of the ocean. In the Atlantic Ocean, on the other hand, we found ourselves, along the east coast of North America, on the inner side of a folded system, the Appalachians, moved towards the interior of the continent. A comparison of the contours of each ocean as a whole will be the task of a special chapter.' (Suess, 1885, pp. 772-773).

In the second volume, Suess reviews the geology of the margins of the Atlantic and the Pacific Oceans in chapters 2 and 3, respectively. The third chapter is a summary and a characterisation of the two distinct types of margins. This is how Suess summarises their differences:

'*With the exception of the cordillera of the Antilles and the mountain fragment of Gibraltar, which form respectively the boundary of the two mediterranean seas, the outer side of a folded range nowhere determines the outine of the Atlantic Ocean.* The older folded ranges which extend from Maine to Newfoundland turn, it is true, their outer side to the lower St. Lawrence and the strait of Belle Isle, but where they reach the great Ocean they disappear beneath it. *The inner sides of folded ranges, jagged rias coasts which indicate the subsidence of Mountain chains, fractured margins of horsts, and fractured table-land form the diversified boundary of the Atlantic Ocean*.

The same structure also characterizes the coast of the Indian Ocean as far to the east as the mouths of the Ganges, where the outer border of the Eurasian chains meets the sea. The Erythraean trough, the fracture of the Quathlamba in Natal, as well as that of the Sahyádri in India, and the structure of Madagascar, the faults of which have been recently described by Cortese [1856-1936], indicate that the structure of this region has been determined by tabular fractures. It is only in the Persian Gulf that some of the outer Iranian zones reach the sea.

The west coast of Australia likewise exhibits Atlantic structure. From the arrangement of the folded ranges we know that they turn their convex side to the Pacific Ocean, and a

[§§§§§§] Baja California of Mexico.

******* The present federal state of California in the United States of America. The Alta California of the colonial Spanish administration.

comparison with South America shows that from the continent of Australia as far as New Zealand and probably New Caledonia, a more or less concentric system of folds is present, turned towards the Pacific side.

The west coast of Australia thus presents much the same relations as that of the east coast of Brazil.' (Suess, 1888, pp. 258-259, emphases are Suess').

Here Suess' interpretation of the Atlantic-type margins connects with his 1873a abstract! He sees in the Appalachians that the ocean is located on the back side of the mountain, i.e., it is in its hinterland. But he knew from his studies prior to 1873 that the inner sides of mountains, i.e. their backs, are commonly sites of fracturing, extension and subsidence. That is where the blood wells up in his much quoted and ridiculed, yet, we now know, incredibly apposite simile. Elsewhere along the Atlantic margins, whole mountain ranges are truncated, fragmented and subsided. This is where rias coasts are seen. His usage of stratigraphy, structure and geomorphology to characterise the continental margin is truly masterly and corresponds precisely to our modern interpretations. In the Dark Intermezzo, the Wegener-Argandians followed Suess in characterising the Atlantic-type continental margins, whereas the Kober-Stilleans did not.

Now let us see how Suess characterised the Pacific-type margins:

'The borders of the Pacific Ocean may be divided for the sake of an overview into five parts.

The first is formed by the *arc of the Aleutian Islands*. While the north of the Atlantic Ocean is characterized by the Archaean massif of Greenland, here on the other hand is swung an island arc, affording with its folded Mesozoic beds and its inner zone of active volcanos a complete contrast of structure. The syntaxial angles of the arcs in the North Pacific Ocean at once recall the relations of the mountain chains of India [Fig. 48].

The second part consists of the *west coast of North America*, from the Gulf of Kenai down to the coast of Mexico. The Queen Charlotte Islands are regarded by Canadian geologists as an outer chain of the Cordilleras.

The third part is *South America*; it begins in Guatemala, where the Cordillera of the Antilles strikes across Central America, is divided into two by a syntaxis in the gulf of Arica and prolonged in an arc to the south beyond Cape Horn. This part is characterized by the coast Cordillera, where the stratified series does not appear to begin till the Neocomian; it recalls in many respects the coast chains of California, as well as the Nicobars and Andamans, which have a similar structure.

The fourth part is formed by the arcs of eastern Asia, which we have recognized in many of their most important segments as the recurved extremities of the great chains of central Asia [Fig. 49]; they are not syntaxial after the manner of independent arcs, but lie one behind the other, forming the termination of a series of chains folded in the same direction and belonging to a single system. The great Malay arc marks the southern border of this system. Timor and Soemba lie outside it; whether these two islands should be included in the Australian region must for the present remain undecided.

As the fifth part we regard the *Australian chains* together with New Zealand and New Caledonia. An opportunity has already presented itself of comparing the Australian chains of the Flinders and Adelaide ranges as far as the east coast with the meridional sierras in the west of Cordova. The absence of middle Tertiary deposits over the whole of the eastern coast of Australia and Van Diemen's Land[†††††††], in contrast to their rich development on the south coast of Australia and in Bass strait, leads to the supposition that the continent east of the existing coast has only in comparatively recent times subsided to the great depths which are now known to occur in this region.

With the exception of a part of the coast of central America in Guatemala, where the bending Cordillera of the Antilles has sunk in, the whole border of the Pacific Ocean, wherever it is known in any detail, is formed of mountain chains folded towards the Ocean in such a manner that their outer folds either form the boundary of the mainland itself or lie in front of it as peninsulas and island chains.

No folded range turns its inner side to the Pacific; no tableland reaches the shores of this Ocean.' (Suess, 1888, pp. 259-261).

But Suess did not leave it here! If the Pacific margins were overthrust margins on a grand scale, what could be the relations of the underthrust sediments to the magmatism that so remarkably accompanies the Pacific-type margins. Suess pointed out in the last volume of the *Antlitz* in 1909 (p. 676) that his colleague in the University of Vienna, Friedrich Becke (1855-1931), published in 1902 and 1903 that two types of

Figure 49: Suess' schematic cross-section across Asia (also see Şengör et al., this volume).This is a section combined from two sections which appear in the same unnumbered plate from which the previous figure was taken. Suess may have drawn them as one section and later cut it into two to comply with the space exigencies, or Sollas may have done so. I have only enlarged he lettering to improve legibility. Notice the 'Foreland sinking under folded chains' in the right. This figure must have been sent to Sollas before Suess died in 1914. Its modern look is breathtaking.

[†††††††] This is present-day Tasmania. Named by the discoverer Abel Janszoon Tasman (1603-1659) after the governor-general of the East Indies Anthony van Diemen (1593-1645) of the Dutch Empire as Anthoonij van Diemenslandt. The name changed to Tasmania only in 1856, but many Europeans, Suess included, continued to use the old Dutch colonial name.

young volcanic rocks had to be distinguished: an Atlantic- and a Pacific-type. This distinction had already been made in essence before Becke: in 1892 the American petrographer Joseph Paxson Iddings (1857-1920) first noticed the distinction in his paper, where he introduced the concept of consanguinity within petrographic provinces, but it was the British petrographer Alfred Harker (1859-1939) who first elaborated on it in 1896 and showed that Iddings' alkali and sub-alkali (now called calc-alkalic or calc-alkaline) groups occurred mostly in the Atlantic- and Pacific-type of continental margins (Harker said 'coast lines') of Suess respectively, thus explicitly citing Suess' distinction of the tectonic environment. He accordingly suggested the terms Atlantic and Pacific 'tribes.' Harker re-emphasised the distinction 1909 in his famous book on the *Natural History of Igneous Rocks* (Harker, 1909, especially pp. 92-93). Suess here refers only to Becke, who is also cited by Harker (1909, p. 93, footnote 1) because, most likely, he discussed the issue with him. Suess wrote (1909, pp. 677-678) that Becke did not distinguish these two types only on the basis of their geographic distribution, but also on a tectonic basis: the Atlantic type was characteristic of regions of inbreaks ('Einbruch') resulting from the radial element of contraction and the Pacific type was typical of regions of tangential shortening ('tangentialer Zusammenschub'). Finally Suess says that Becke's hypothesis is based on the assumption that in the Pacific magmas there is a considerable assimilation of sedimentary rocks, which would explain the greater content of Ca and Mg (Suess, 1909, p. 679).

This remarkable hypothesis was also almost forgotten until 1953, when Stille reinvented it while working on his monograph on the tectonics of the Carpathians (1953, pp. 184-193; also see Stille, 1954) and later applied it to the active circum-Pacific magmatism (Stille, 1955, 1960). I assume he reinvented it because he gives no reference to Suess and during the International Geological Congress in Algiers in 1952, when he presented this idea, not one geologist who took part in the following discussion (Ernst Kraus, Germany; Wilhelm Petrascheck, Austria; Emil Tröger, Germany; Silvio Vardabasso, Italy; Paul Ramdohr, Germany; Louis Glangeaud, France: see Stille, 1954, pp. 136-137) thought of reminding Stille that Suess had already said it half a century earlier in no equivocal form. So dark was the Dark Intermezzo!

[29] This is the Tethys, which Suess had christened in 1893b. As I have at length dealt with the history of this concept in Şengör (1998), I shall not here repeat what I said there, except to show Fig. 50. It illustrates one possible and possibly a very conservative interpretation of Suess' idea of what the structure of the Tethys may have looked like. He said that oceans originate by coalescing elliptical subsidences (Suess, 1888, p. 681) and gave the Neogene to Recent history of the Mediterranean as an example of how oceans originate (Suess, 1909, p. 722). He gave the phenomena of faulting and vulcanicity in Iceland as an example of how the building of the Atlantic was proceeding in our own day by downfaulting and volcanism (Suess, 1888, p. 681).

In Suess' world oceans were destroyed by shortening. He cited only two examples of 'dead' seas not destroyed by tectonic events: The 'dead' sea of the El Djouf desert in the western Sahara and the 'dying' Caspian Sea (Suess, 1909, p. 747). As I showed above, Suess was among the first to appreciate the importance of asymmetric horizontal motions in mountain-building. The implication of these ideas, that marine basins originate by normal faulting and subsidence and mountains originate by shortening such basins, was that originally raceme-shaped map views of the oceans (Fig. 51) were transformed into the linear/arcuate mountain belts (Fig. 51) by the subsequent intense shortening and that the final map-views of mountain belts had little relation to the original outlines of the oceans out of which they had grown (cf. Suess, 1888, p. 681). Suess emphatically rejected the theory of geosynclines, the alleged mother troughs of mountain ranges, that supposedly initially had the linear/arcuate aspect of their offspring (Suess, 1888, pp. 263-264, 1909, pp. 722; Suess to Ruedemann quoted in Kober, 1928, p. 51). The difference between Suess' thinking and that of the geologists favouring the geosyncline idea resulted from a difference in their interpretation of the tectonic expression of the contraction theory: Suess considered oceans to result from a radial component of the contraction dominated by steep faults, whereas the mountain ranges to be built by its tangential component resulting in folds and shallowly-dipping thrust faults (Suess, 1875, pp. 146-149, Figs. 42-47, 49, 51). His opponents thought that the marine basins, i. e., what they called 'geosynclines' and the eventual demise of geosynclines under shortening, were both caused only by the tangential component of the contraction. This was nothing but a wholesale return to Élie de Beaumont's theory.

The Tethys was a product of coalescing elliptical subsidences and the resulting ocean probably had little resemblance to the lie of the mountain range its destruction eventually gave rise. Fig. 51 A-C shows the probable stages Suess had in mind during the destruction of an ocean in general and Tethys in particular.

[30] See von Richthofen, F. (Freiherr) (no date, 1877, 1882, 1883, 1907, 1911, 1912).

[31] Harada (1890) is a fine example of the sort of publications Suess probably had in mind. He cites that publication with emphasis in his chapter 4 of vol. III/1 of the *Antlitz* (see his endnote 55 there).

32 For a complete list of the studies ('Trudi') undertaken along the Transsiberian Railway, the legendary *Transsibirsky Magistral*, which Suess used for the first part of the third volume of the *Antlitz* (Suess, 1901) and here refers to, see Comité Géologique de Russie (1900).

[33] The Great Rift Valley was not discovered by Höhnel and Teleki, but it was their expedition that yielded to Suess the keys of its proper interpretation. The topographic feature was long known, but it was Suess who discovered that the whole structure was an extensional one bounded on both sides by normal faults and accompanied by alkalic magmatism. In his memoirs, Admiral von Höhnel (1857-1942) relates how enthu-

siastically Suess greeted their observations and that it was Suess who suggested that the results be considered geologically and the speciments brought back be studied by his colleagues:

' A friendly relationship developed with time between me and Professor Suess out of initially only business-like connexions. Upon his urging, the geographical, petrographical and the geological results of the expedition were worked up in detail for the Annals of the Imperial Academy of Sciences, on which he based his famous graben theory that was new for Africa and since then found general acceptance.' (von Höhnel, 1926, p. 78).

The results were published in von Höhnel (1888, 1889, 1890a, 1890b, 1891a, 1891b, 1892) and von Höhnel et al. (1891) and created great admiration in the international geological and geographical community, but von Höhnel was being optimistic in his assessment of the reception of Suess' ideas on East Africa: Suess' extensional graben interpretation was not immediately accepted, not even by his own students. Suess had already used in 1883 (in the first part of the *Antlitz*, p. 166) the old miner's term 'Graben' to designate areas that subsided between two peripheral faults delimiting round regions of subsidence, one normal, the other thrust (what O. Meyer later called 'graben without extension': see Meyer, 1915, fig. 4; also Krenkel, 1922, fig. 14b). In east Africa, he applied it to sunken areas between two normal faults and this usage was immediately taken up internationally. The British geologist John Walter Gregory (1864-1932), encouraged by Suess, explored the eastern arm of the great African Graben but preferred to call the structure a 'rift', derived from the root 'reve' meaning 'to pull asunder, to tear apart' (Gregory, 1894, p. 295) and strangely thought that it resembled the type of structure exemplified by the Yosemite Valley in California, clearly under the in-

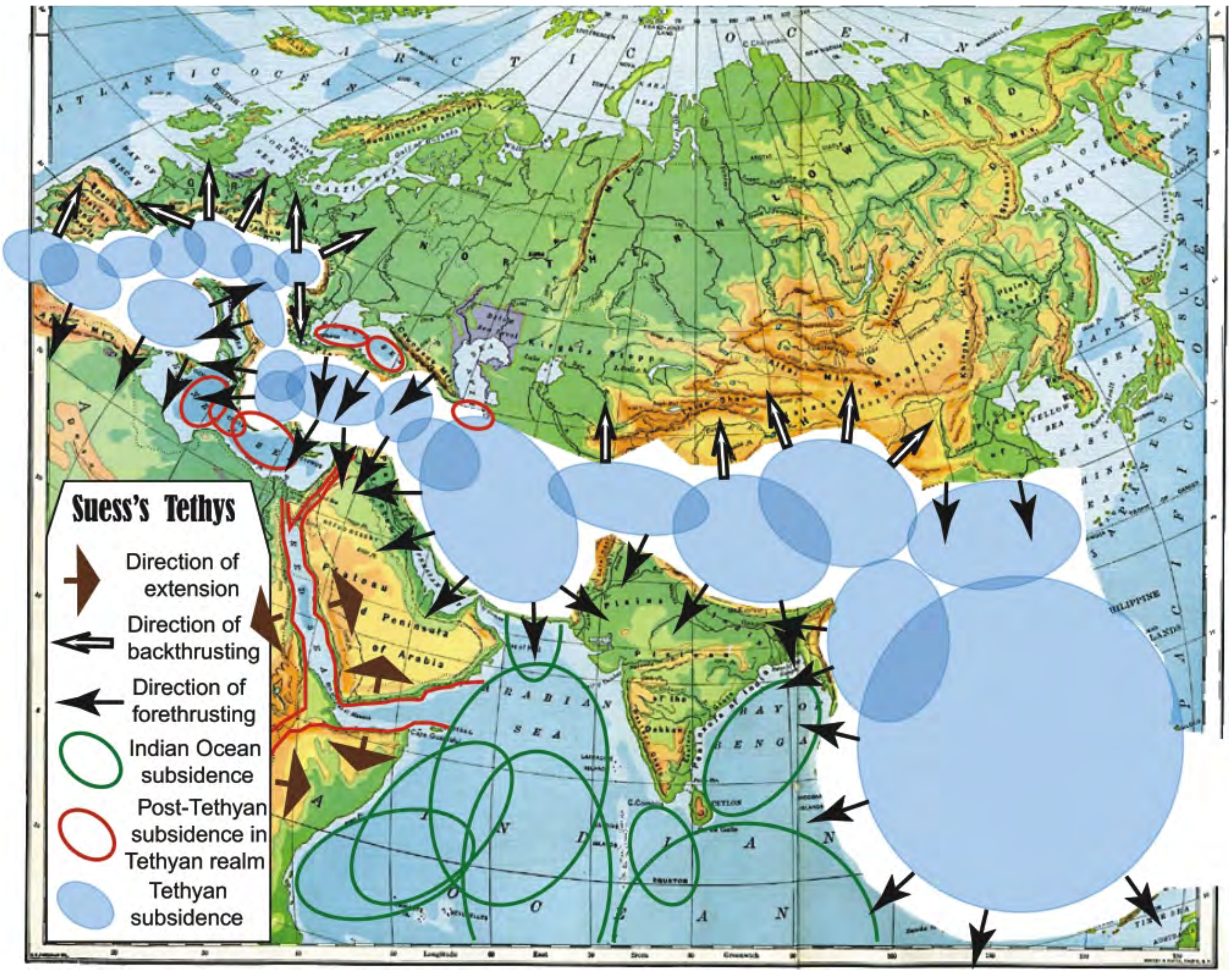

FIGURE 50: A structure of coalescing elliptical subsidences as shown here is what Suess may have imagined the Tethys to have had. After having described in detail how the present-day Mediterranean formed by coalescing subsidences, he stated that 'The manner in which sea basins arise by the coalescence of subsidences is illustrated by the history of the Mediterranean' (Suess, 1909, p. 722). In an associated note, he added: 'The lie of sediments which fill it is possibly synclinal, somewhat as described by Haug, Traité de Géologie, I, 1907, p. 159, fig. 36. This, however, is not the tectonic conception of the geosyncline, and the geanticline cannot be regarded as its opposite; the geanticline was originally conceived by Dana in a different sense, but to many authors both words seem to presuppose equilibrium and the germ of the isostatic theory. For this reason I regret that at first I employed the term geosyncline in this work; subsequently I avoided it' (Suess, 1909, pp. 737-738). The 'backthrusting' of Alpide chains around the Mediterranean is only with respect to Suess' 'Asiatic structure'. From Şengör and Atayman (2009, fig. 2, reproduced with the kind permission of the Geological Society of America, Inc.).

fluence of Josiah Dwight Whitney's (1819-1896) erroneous and rapidly abandoned (except by Whitney himself) interpretation published in 1869 (although the book was dated as 1868). Gregory also returned to Élie de Beaumont's interpretation of a fallen keystone at the apex of a giant crustal anticline that extended from South Africa to Ethiopia, in which he was followed by the French geographer Emmanuel de Martonne (1873-1955), who assumed in 1897 two anticlines separated by a syncline housing Lake Victoria instead of Gregory's one. But de Martonne's countryman Albert-Auguste Cochon de Lapparent (1839-1908) returned to Gregory's view in 1898.

In the early to mid-twentieth century two different trends of thinking on the genesis of rifts competed: One was initiated in 1907 by the great Viktor Uhlig (1857-1911), Suess' student, friend and successor, who thought he could see evidence that the rift margins in east Africa were actually thrust faults and therefore interpreted the whole rift as a compressional structure. Similar ideas had been voiced about the Upper Rhine Rift already in 1887 (published 1892a, 1892b) by Achilles Andreae (1859-1905), who was followed in 1903 by Wilhelm Salomon (1868-1941). Uhlig, however, gave up his idea in 1909b. His self-critique went unnoticed by the shortening enthusiasts led by Edward James (''Jimmy'') Wayland (1888-1966) in East Africa (Wayland, 1921), who thought that he could not explain the impressive shoulder uplift of the rifts without shortening. This idea was supported by the extensive report of Bailey Willis (1936), who however had gone to East Africa to find support for his theory of ramp valley (Willis and Willis, 1929, p. 82, Fig. 50, pp. 93-96) and rumour has it that he wrote his conclusions on board ship while travelling to Africa! Already in the text-book that he had written with his son Robin, in 1929, he had concluded:

'Gregory accepted the suggestion of Suess to the effect that the Rift valleys represent a dominant lineament of the globe, which extends for a distance of 4,000 miles, from Lake Nyassa in South Africa to the Jordan Valley in Syria, and includes the African Rift Valleys, the Red Sea depression, and the Dead Sea trough. This is one of those broad, but speculative concepts that appear reasonably probable so long as they are not embarrassed by too many facts. Suess made his suggestion on the basis of what was known of East Africa at the beginning of this century. His genius for connecting and correlating tectonic lines led him to extremes. The accumulating facts now indicate that the African Rift valleys are neither topographically continuous nor certainly attributable to identical causes, and that the Red Sea and Dead Sea are distinct structural depressions which may or may not have had similar histories.' (Willis and Willis, 1929, pp. 96-97).

Willis was an arrogant man bordering on being a charlatan, who, in his report, written with the greatest self-assurance, among numerous geological errors and confusion, spelled Suess' first name consistently as if he were a Frenchman (although he claimed to have known Suess well; see below), got Uhlig's first name wrong and thought Steno was Italian and finally who called continental drift a Märchen (tale) in 1944 in the title of a paper in which he also went so far as to write the following about Suess 'Fellow scientists who are not geologists cannot be expected to know that the geology upon which the protagonists of the Theory rest assumptions is as antiquated as pre-Curie physics. Wegener and his successors are disciples of Edouard [*sic*!] Suess, the Master of European geologists. I knew him well: a charming genial German who never traveled far, but assembled the observations of others and from them constructed speculations regarding the face of the Earth. His reading was prodigious, his memory marvelous, his imagination grand; but he gravely lacked critical faculty. And when some airy concept had grown in his mind, it became too firmly rooted ever to be dislodged' (Willis, 1944, p. 510)! In his popular book on his travels in Africa Willis reports on a visit to Suess' house in 1912 in Marz and says that Suess was sitting with his wife and daughter (Willis, 1930, p. 1). Now this is impossible, since Hermine Suess, Suess' beloved wife, had been dead since 1899 and there is no record of Suess' having married again![‡‡‡‡‡‡‡] Fortunately, history soon showed who actually lacked critical faculty and who could never change his firmly rooted 'airy' dreams!

In 1936, the shortening theorists for the rift valley could enlist Sir Edward C. Bullard's (1907-1980) support, who thought that the large negative gravity anomalies over the rift indicated a mass deficiency at depth which he ascribed to a depressed crustal block overthrust from both sides. Unlike Willis, however, Bullard was a great scientist and a reliable author.

The most difficult thing to accept was to have such a large allegedly extensional structure on a supposedly contracting planet! Suess said it was extension by contraction ('Zerrung durch Contraction': Suess, 1909, p. 316) and nobody could understand what he meant. The only two ways to extend the earth's crust by contraction would have been the following:

1) At the initial stages of the contraction when the newly cooling crust would try to contract on a hot interior and would be stretched in the process. This had been pointed out already by Count de Buffon (1778, p. 74) and Delamétherie (1795, p. 367) and Cordier (1827) followed their example.

2) By building anticlinal crustal bulges and giving rise to extrados extension at their tops creating keystone-like structures. This was a way first mentioned by Leopold von Buch (1820) and later developed for the Upper Rhine Graben by Élie de Beaumont (in Dufrénoy and Élie de Beaumont, 1841, pp. 436-437; see also Şengör, 2003).

The first was impossible, because according to all contraction theorists, including Suess himself, the earth had long past the stage during which the sorts of extensional structures men-

[‡‡‡‡‡‡‡] I relate these details just to show how reliable any account by Willis may be. Many years ago, during a visit to Stanford University, I told the late Benjamin M. Page (1911-1997), who had known Willis personally, that I had the impression that Willis was a charlatan and asked him what he thought. In his gentlemanly manner this fine geologist and kind man simply said 'I think so too.' For Ben to be able to say that he must have really had a very strong impression in that direction.

tioned by de Buffon and Delamétherie would form. The second possibility was not open to Suess, because his whole tectonic theory was based on the denial of primary vertical uplifts and he had conceived the *Antlitz* as a long refutation of the primary uplift theory. Once these two possibilities were eliminated, his readers could not see how extension could possibly be generated by contraction.

But there is a third way, namely that of membrane stresses, first used in a plate tectonic context by Turcotte and Oxburgh (1973) and Turcotte (1974) and applied to the East African Rift by Oxburgh and Turcotte (1974). Solomon (1987) pointed out that they can give rise to rifting on a contracting planet. Already the German astronomer Jakob Karl Ernst Halm (1866-1944; see Spencer Jones, 1945) had used the membrane stresses to argue that rifting could take place on an expanding earth (Halm, 1935). Whether rifting or extension will take place on a contracting or an expanding planet would depend on the shape of the stressed brittle caps located within its lithosphere. Fig. 52 shows how rifting could take place on a contracting planet through membrane stresses directly as a consequence of contraction; in other words, how extension through contraction is not only possible, but has been actually suggested within the framework of plate tectonics precisely for that structure for which Suess had originally proposed it[§§§§§§§].

Although Ludwig Prandtl (1875-1953) first suggested the membrane analogy for the study the deformation of thin elastic sheets in 1900 and the theory was developed by his student H. Anthes in 1904 (Timoshenko, 1953, p. 393), it is not necessary for Suess to have known of these engineering developments, although, as the President of the Academy of Sciences in Vienna, he easily might have. The reason for this is that the situation illustrated in Fig. 52 is so geometrically obvious and particularly applicable to situations where the lithosphere may be segmented as it was in Suess' world.

Suess could have also supposed that the continents were spreading under their own weight creating shortening around them and extension on their tops, as he believed that rocks flowed at a continental scale and caused deformation (Suess, 1896, 1904c). It was this idea that so much influenced Frank Bursley Taylor (1910, 1921, 1928a, b, 1930, 1933) and Émile Argand (1912, 1916, 1920, 1924, 1928). But this would not be 'extension through contraction' and that is why I do not think this is what he meant.

[34] Lóczy (1893) provided data for Suess particularly for eas-

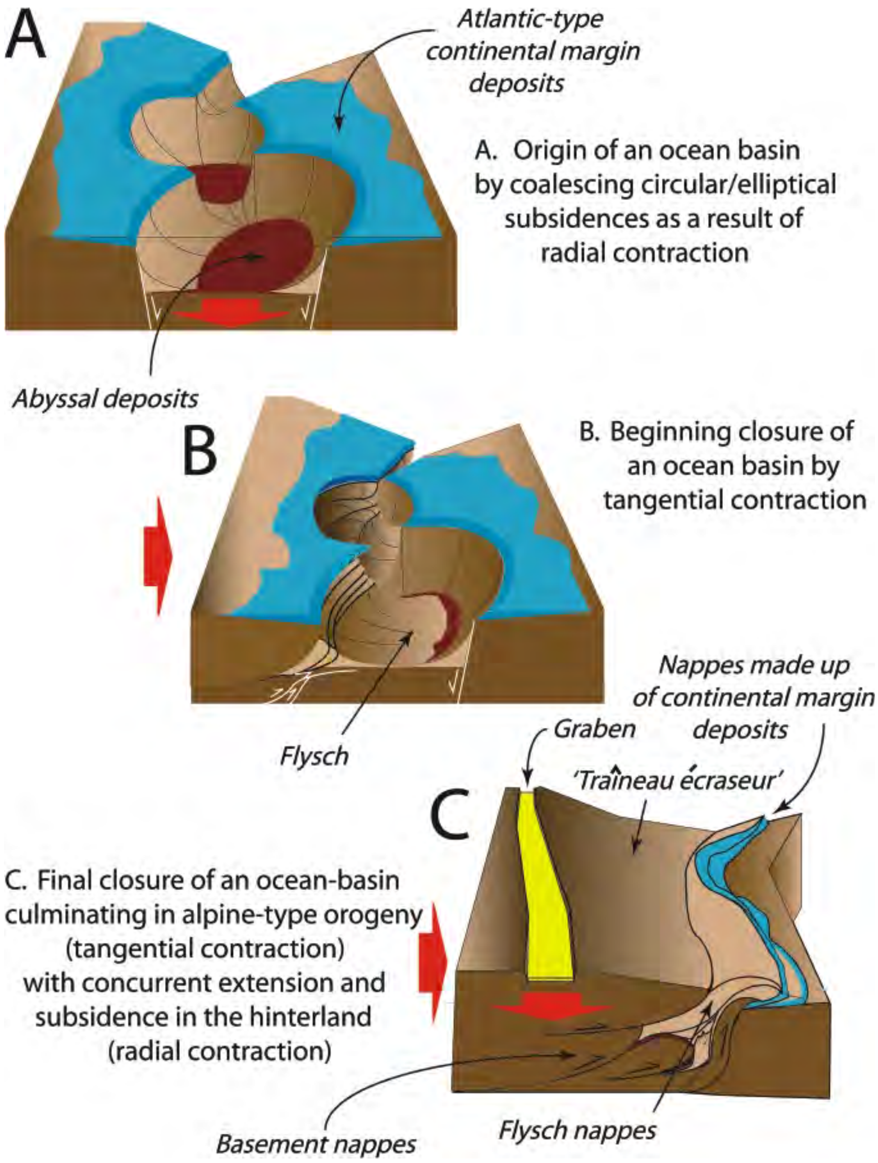

Figure 51: How coalescing elliptical subsidences forming ocean basins (A) may be converted into a linear/arcuate mountain belt (C) according to Suess' version of the theory of thermal contraction of the earth. A raceme-shaped map-view shown in A is progressively converted into a narrow, linear/arcuate orogenic belt by progressive shortening (B and C). As far as we know, Suess was the only one who did not think that the mother-basins of mountain belts had to have any map-shape similarity to their orogenic decendents. It was unfortunate that geology did not follow this important insight for nearly a century because of the blinders placed around its vision by the geosynclinal theory. The three time-lapse figures were drawn using Suess' descriptions in Die *Entstehung der Alpen* and *Das Antlitz der Erde*. (From Şengör and Atayman, 2009, Fig. 3, reproduced with the kind permission of the Geological Society of America, Inc.).

[§§§§§§§] I must point out, however, that Dickman and Williams (1981) pointed out that the effective viscosity of the lithosphere is too small to store the stresses resulting from the radius changes of the lithospheric plates.

tern Kuen Lun and thus complemented Bogdanovich (1892).

[35] Obruchev (1900, 1901)

[36] Of all the others, Bogdanovich (1892) was very important to give Suess access to northern Tibet and the Kuen-Lun, which were till then a complete geological and even geographical *terra incognita*.

[37] Suess here means the discovery of the great nappes in Switzerland by Bertrand (1884), Schardt (1893, 1898) and Lugeon (1902). Sir Edward B. Bailey (1881-1965) informs us that Bertrand was influenced by Suess in his discovery:

'Marcel Bertrand, already in 1884, was ripe in the experience that early comes to genius; and thus he was able to replace Escher's Double Fold by a northward single fold (modified by thrusting) that moved in company with the underlying structures [Fig. 53]. It was unnecessary for Bertrand to visit the Glarus exposures, so beautifully portrayed by Heim [the reference here is to Heim's magnificent monograph on the Tödi-Windgällen Grup: Heim, 1878a, b, c; see especially plate VII in Heim 1878c]. He had learned from Suess' *Entstehung der Alpen*, published in 1875, the tendency for mountain structures in any particular area to show consistent movement in one direction—in the Swiss Alps that direction is more or less north-northwest. He may possibly have noticed the significant absence of any reference to the Glarus Double Fold in Suess' *Antlitz der Erde*, which began to appear by instalments in 1883.' (Bailey, 1935, p. 52)

It is very interesting that Albert Heim (1849-1937), the Nestor of Swiss geologists at the time and a friend of Suess, credits Suess together with Bertrand for the discovery of the Glarus Nappe. Sir Edward tells us why:

'We have already pointed out that until 1891 Heim regarded Bertrand's contribution to Glarus tectonics as a negligible quantity. All this time, however, Suess had been watching. In the first volume of his *Antlitz*, 1883-5, he purposely refrained from mention of Glarus. In the second volume, 1888, he quoted, without comment, Bertrand's comparison with the Belgian coal field. Finally in 1892 he visited the exposures and, returning by Zurich, he expounded to Heim, on the latter's beauitful relief model, the true interpretation. Heim has told me that he urged Suess to publish his views and the evidence upon which they rested. The latter replied: "No, I shall not. You must do it yourself, if you can agree with the idea." So it comes about that Heim in his *Geologie* places two names under the revised section that he draws across Glarus: Bertrand, 1884 and Suess, 1892 [Fig. 53]. This acknowledgement is fitting, for Suess, before any one else, recognised the northern Alps as a one-way route for tectonic purposes, and he had in his own mind corrected Escher's Double Fold. If he had not refrained from publishing, there can be little doubt that investigation of Alpine problems would have been greatly accelerated.' (Bailey, 1935, p. 54).

Here is another example of Suess' incredible modesty, his sincere consideration for other people and his immense generosity. He could have received credit for discovering the critical evidence in support of Bertrand's interpretation. He preferred, however, that the local expert Heim do it. He must have also thought that if Heim could convince himself in favour of Bertrand's view, its impact would be so much more. Suess was not only modest, considerate and generous, but also extremely intelligent, a shrewd reader of the human mind.

[38] Alfred Elis Törnebohm (1838-1911) documented in his 1896 book the for then incredible horizontal transport distance of nearly 200 km of the south Norwegian Caledonide nappes. This, and his own observations in the far north of Scandinavia between Kvikkjok and the Norwegian coast (Holmquist, 1900) led Per Johan Holmquist (1866-1946) in 1901 to claim that the edge of the Baltic shield had to be under the middle of the North Sea, ten years before Ampferer and Hammer came up with the concept of *Verschluckung* in the Alps (see Şengör, 1977). As I showed in my 1977 paper, Suess not only cited Holmquist and reproduced his figures in the first part of the third volume of the *Antlitz*, but also very much approved of this (Suess, 1901, pp. 494-496, figs. 20-23).

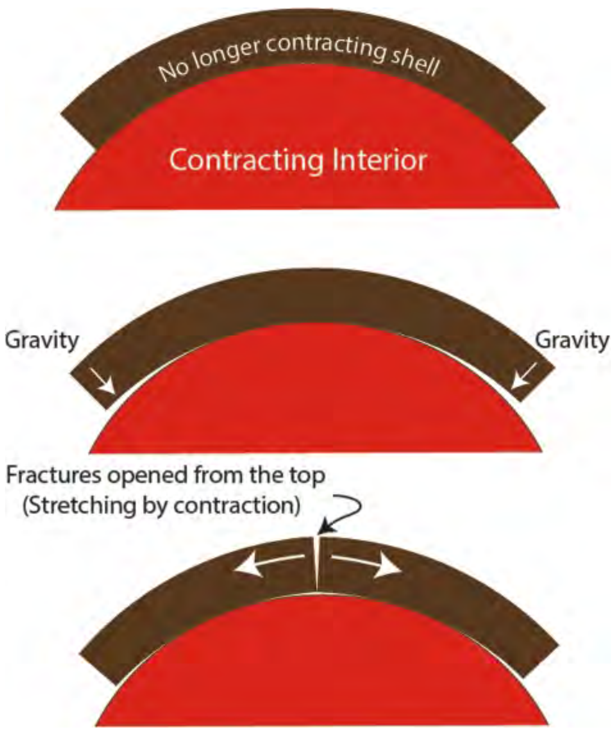

Figure 52: Extension through contraction (*Zerrung durch Contraction*). In the uppermost figure, no contraction has yet occurred. In the middle figure, the hot interior contracted, but not the overyling already cooled plate. In the bottom figure, the ends of the non-contracting plate fall towards the interior because of gravity and shorten peripherally while leading to extension in the middle (see Solomon, 1987).

4. Concluding remarks

We have reviewed two texts by Suess with a view to using them as guides to understand his fundamental ideas in geology: his 1873(a) abstract in which he discussed the structure of what he called Middle European High Mountains including the Apennines, Dinarides, Carpathians, the Alps and what we today comprehend under the 'Hercynian Orogen' plus some of the Cretaceous-Cainozoic fold and thrust bundles in the

Alpine foreland of Europe, and his 1904 letter to Professor William Johnson Sollas which serves as the author's preface to the English edition of the *Antiltz*. I presented an exegesis of these two texts by pointing out their background and explicating Suess' main messages. Two main points emerged from this exegetical exercise:

1. Mountain-building was for Suess an asymmetrical event, i.e., caused by one lithospheric block overriding the other because of independent motions of internally rigid to semirigid entities. Shortening formed folded and thrust mountain belts in front of such blocks while extension characterised their backs. Such motions are not usual in theories of global contraction, so Suess had to assume different depths of detachment for his moving blocks. Only locally they were fixed to their substratum. In places he thought the upper portions of the earth had a greater tendency to horizontal motion than their substrata. None of these ideas found any echo in the thought of his successors, except in those of his son Franz Eduard Suess, the last of the Viennese giants, who combined his father's ideas with Wegener's continental drift. The resulting tectonic picture for the continental deformation and the structure of orogens is not just similar, but identical to what has been developed after plate tectonics was invented (see especially his *Bausteine zu einem System der Tektogenese*: Suess, 1937, 1938, 1939, 1949). The son Suess' work was totally ignored and became forgotten in the darkness of the Intermezzo that separated the publication of Argand's *La Tectonique de l'Asie* (Argand, 1924) from Tuzo Wilson's invention of plate tectonics in 1965.

2. Eduard Suess' work on litho- and biostratigraphy and transgressions and regressions showed Suess that global stratigraphy only made sense if one tied it to great physical changes in the structure of the planet somehow causing the sea-level to move vertically with respect to the continents generating unconformity-bound sequences that could be correlated globally. He noticed that this had to be explained not by independent vertical continental motions, but by somehow changing the capacity of the ocean basins from time to time. The only possible mechanism he could find was Constant Prévost's version of the contraction theory, in which ocean basins, or parts thereof, subsided along major faults at continental margins or within the ocean floors because of unequal contraction creating new ocean volume into which the oceanic waters retreated. This gave rise to regressions. As the created holes were later filled with sediment again, the same waters this time were irrupted onto the continents causing transgressions. Regressions and transgressions also caused extinctions and explosive diversifications in the history of life by eliminating and enlarging shelf seas. Suess was so impressed with this mechanism that global contraction appeared to him as the *sine qua non* of global tectonics, although in the last volume of the *Antlitz*, he expressly noted that contraction alone did not seem sufficient to cause the immense shortening seen in mountain ranges (Suess, 1909, p. 721). In 1911, in a letter he wrote to Charles Schuchert in Yale, he also said that polar wander could not be explained within the framework of the contraction theory (Suess, 1911, p. 107). But in 1913, he warned against too enthusiastic an abandonment of the contraction theory; it did not

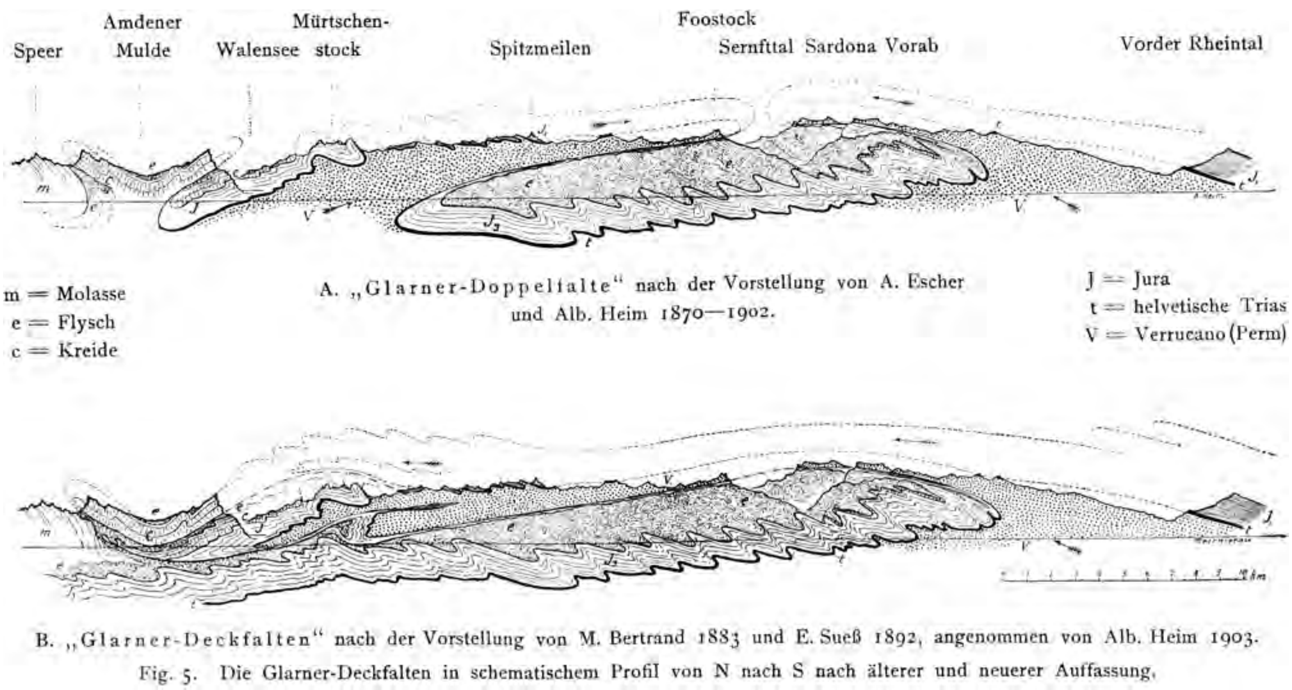

Figure 53: Heim's famous drawing of the old and new interpretations of the geology in the Swiss canton of Glarus. Above is the famous Glarus Double Fold of Escher and Heim (1870-1902). Below is Marcel Bertrand's reinterpretation of 1884 as a single north-vergent nappe. Notice, however, that Heim also credits Suess here! Translation of the legend: A: "Glarus Double Fold" according to the view of A. Escher and Alb. Heim 1870-1902. Amdener Mulde=Amden Syncline, Kreide=Cretaceous, Sernfttal=Sernft Valley. B: Glarus Nappe Folds according to the view of M. Bertrand 1883 and E. Suess 1892, accepted by Heim 1903.

explain many things, yet it explained so many!

For all the confidence he had in his contracting earth model, he was even dissatisfied with the explanation he had to devise for transgressions. In 1911, in his letter to Schuchert he wrote: 'When I wrote of eustatic movements in 1883, I confessed that I did not understand the transgressions. I thought that variations in rotation might somehow have influence. I also believed and still think that the accumulation of sediment was a *vera causa*, but hardly sufficient. Now, after twenty-seven years, I cannot offer you more than a heap of doubts regarding the explanation. I have learnt more and know less about it.' (Suess, 1911, p. 107).

Interestingly, he had no problem with major extensional structures such as the East African Rift Valley and he very ingeniously accommodated them in the framework of a contracting earth using ideas that resurfaced only in the seventies of the twentieth century (with one exception in the thirties)!

The conceptual edifice Suess left behind was grand, yet incomplete and he had no intention of deceiving himself that he could complete it. The oceans were almost totally unknown. He used whatever information that could be gleaned from the results of such oceanographic expeditions as that of the *Challenger*, but all they provided was a crude bathymetry and dredge material. Ocean islands were known and it was appreciated that almost all of them were basaltic in composition.

Another major problem Suess had was his aversion to isostasy. He could not accommodate it in his picture of a contracting earth. He called himself a 'heretic' in that regard (Suess, 1911). Among his successors in the world of tectonics, Wegener and Argand synthesised isostasy with Suessian tectonics and the result was continental drift.

The success of Suess' synthesis on the continents is baffling. Almost everything he said about continental structure, including the nature of the rift valleys and continental margins, found themselves a ready home in plate tectonics, although much of what his fixist contractionist successors said had to be discarded.

I have often pointed out that Suess' great success was due to six factors: 1. His critical rationalist attitude; he never fell in love with any of this own theories and used the evidence to test them as mercilessly as possible. 2. His refusal to assume regularities on empirical data and his unwillingness to abandon Lyell's way of looking at the planet. He always interpreted the past using present phenomena. The history of the earth was not uniformitarian in the sense of an absolutely steady-state earth, but it was uniformitarian in the sense that the processes have not changed. This is what Lyell also said, although he is often misrepresented by many of the modern historians of geology who are not geologists (and by a few who are, or used to be, geologists). Suess acknowledged that Nature was irregular and pretty much uniformitarian in its behaviour as far as geological phenomena are concerned. 3. His avoidance of becoming a specialist: he spanned the entire spectrum of the earth sciences. His research was not subject-, but problem-driven. He got interested in problems and attacked them with whatever weaponary he could muster. 4. His incredible network of informants throughout the world (in this regard too his way of working resembled Darwin's). Suess embraced the entire planet from the first day of his entrance into geology. He knew that, however well-executed, local studies always have the potential of misleading the geologist concerning general problems. He was trilingual from childhood, but throughout his professional career he increased the number of languages he could read and speak, finally, at age 70, incorporating even Russian into his list of languages. 5. His command of the literature was amazing. He seems not to have neglected even the smallest brochure about an area or about a problem he was interested in. He read all the expedition repors about areas on which he was working, even if they contained only trifling amounts of geological data. Suess made sure that he had as complete a mental picture as possible of the areas he was studying. The way he describes landscapes he never saw is astonishing. I have wandered in many of the places he described: in Europe, in Asia, in Africa and in North America. I have always admired how accurate his depictions of those areas were and how much he was able to reflect the 'spirit' of the terrain. 6. Finally, and perhaps most importantly, Suess was genuinely interested in knowing and understanding. He was an information absorber and a knowledge generater on a hitherto unseen scale in geology. He had no interest in being considered by others to be right or being regarded as an authority. He never fished for recognition, but was always hungry for knowledge.

He was a genuinely modest person; perhaps too modest. Had he been somewhat less modest, he might not have been forgotten to the extent that he has been, even in his own country. When the City of Vienna recently decided to change the name of the Dr.-Karl-Lueger-Ring, it simply renamed it as Universitätsring although Suess' last office is right on it. When the City was approached with an invitation to take part in the celebrations at the occasion of the centenary of the death of the master, its Cultural Office expressed little interest, although those who declined to be interested every day drink the water Suess provided for them.

Whatever the appreciation Eduard Suess receives from his fellow geologists and human beings, there is no doubt that geologists today stand on his shoulders and millions of people benefit from his humanitarian work as a politican. He was a very great scientist and perhaps even a greater human being. But his work is not easy to read and the main purpose of this paper is to show what sort of effort is needed to understand what he says. This is not his fault. Earth is not a simple object. No planet is. We cannot sit back, 'consider a spherical earth' and think that that's all there is to it.

Acknowledgements

I thank Michael Wagreich and Franz Neubauer for inviting this contribution. B. Clark Burchfiel, Werner Piller, Şevket Şen, Fritz Steininger and Hans Thybo were as usual ready with help and good advice. The great head librarian of the

Geologische Bundesanstalt in Vienna, Thomas Hofmann, and another librarian and also a friend Mallory Stark in Harvard, helped me with some library research. Nalan Lom provided efficient help in the office and checked the final reference formatting. I am grateful to my wife Oya, for her great support while spending yet another spell alone, facing the dreadful political and social events taking place in our unfortunate country, while I was away in my library enjoying my life in conversation with Suess. Two reviews, one by my scholarly historian friend Johannes Seidl, to whom I owe many a precious piece of information about Suess and the other, by a geologist friend, Michael Wagreich, greatly improved the presentation of this paper and helped me avoid some embarrassing errors. The end result is, however, enitrely my responsibility.

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Received: 5 March 2014
Accepted: 20 June 2014

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Appendix

Das w[irkliche]. M[itglied]. Herr Prof. Suess legt eine Abhandlung vor, betitelt: ‚Ueber den Aufbau der mitteleuropäischen Hochgebirge'. Es wird zunächst gezeigt, dass die bisherige Ansicht von der symmetrischen Structur der Hochgebirge und ihrer Erhebung durch eine zentrale Axe aus vielen Gründen nicht mehr haltbar sei, vor allem aus dem Grunde, weil eine nähere Betrachtung zeigt, dass mit Ausnahme eines kleinen Theiles der Alpen und vielleicht des südlichsten Theiles der italienischen Halbinsel überhaupt südliche Nebenzonen an den mitteleuropäischen Gebirgszügen nicht vorkommen. Die neueren, von der normalen Einseitigkeit der Gebirge ausgehenden Erklärungsweisen, wie jene von Dana und Mallet, entsprechen wohl der Sachlage besser, reichen aber ebenfalls nicht hin. Die Alpen gabeln sich nicht, wie gewöhnlich gesagt wird, in der Bucht von Gratz, sondern die mitteleuropäischen Gebirge bilden in ihrer Gesammtheit vom Appennin bis zu den Karpathen eine Gruppe fächerförmig aufeinander folgenden Ketten, welche gegen Nord oder Nordost regelmässige Faltungen, an der entgegengesetzten Seite aber Zerreissungs- und Senkungsfelder, vulkanische Gebilde und Erdbebencentra zeigen.

Die erste dieser Fächerförmig aufeinanderfolgenden Ketten ist die italienische Halbinsel, die zweite Gruppe bildet Dalmatien mit dem Karst und den Bosnischen Bergen, die dritte Gruppe die mehr und mehr ostwestlich streichenden croatischen, dann die südsteirischen Ketten, die nächste, schon mit südwestlichen streichen der Bakonywald, die letzte endlich die grosse Kette der Karpathen.

Die Alpen selbst sind als mehrere aneinandergeschobene Ketten anzusehen, wie dies sehr deutlich der isolirte Streifen von Triasgesteinen in Kärnthen beweist.

Ebensolche Ketten sind der Jura und die schwäbische Alp.

Alle diese Gebirge sind in ihrem Verlaufe von der Lage älterer Gebirgsmassen abhängig und ihre Stauung an den alten Gebirgsmassen ist nicht nur im französischen Jura, im schweizerischen Jura u. zw. am Südrande des Schwarzwaldes oder in dem Verlaufe der Anticlinalen der österreichischen Kalkzone südlich von der böhmischen Masse erkennbar, sondern ist die ganze bogenförmige Umgebung der einzelnen Ketten der Westalpen, deren Zusammenhang Desor richtig erkannte, als eine Stauungserscheinung anzusehen.

Wenn die alten Massen von Sardinien mit Corsica und den Hyeren, von Mittel-Frankreich, Mittel-Deutschland und Böhmen als Inseln angesehen würden, und es würde ein Meer den Zwischenraum ausfüllen, dessen Fluthwelle aus Südwest einsetzt, so würde der Verlauf dieser Welle jenem der grossen Kettengebirge durchaus ähnlich sein.

Die alten Gebirge selbst scheinen stellenweise zu zerreissen und einer ähnlichen Richtung zu folgen, so das Riesen- und Erzgebirge. Weit im Osten folgen die Kettengebirge ähnlichen Gesetzen, so der Balkan, dessen Trachytkette schon von Hochstetter mit den Basalten des Riesengebirges, den Trachyten der Karpathen und den Vulkanen Italiens verglichen wurde, so auch der Kaukasus mit der Scholle an der Südspitze der Krim.

Der Verfasser gelangt zu dem Schlusse, dass die gesammte Erdoberfläche sich tatsächlich in einer allgemeinen, aber überaus langsamen und ungleichförmigen Bewegung befindet, welche in Europa zwischen dem 40. und 50. Breitengrade gegen Nordost oder Nord-Nordost gerichtet ist. Die sogenannten alten Gebirgsmassen bewegen sich dabei langsamer als die zwischen ihnen liegenden Regionen, welche Ketten bilden, die sich aufstauen und in welchen in Mitteleuropa an der polaren Seite regelmässige Falten, and der aequatorialen aber Risse erzeugt werden.

Diese eigene Bewegung der Erdoberfläche verhält sich zur Bewegung der ganzen Planeten etwa so, wie die sogenannte eigene Bewegung der Sonnenflecken zur Rotation des gesammten Sonnenkörpers und ihre Richtung ist in verschiedenen Theilen der Erdoberfläche eine verschiedene.